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HEREDITY OF FEEBLE-MINDEDNESS.

BY HENRY H. GODDARD, PH.D.

(*Read April 19, 1912.*)

In formulating the laws of human thought, the logicians recognized what they called the fallacy of too few heads of classification. This might be called the fallacy of ignorance or of immaturity. It is characteristic of immature minds, the child mind and the beginnings of any science. To the child who has just learned the meaning of "papa" and "mamma," it often happens that all men are "papas" and all women are "mammas," while the child born in certain localities believes that all men are either white or black. Similarly in the beginnings of science, we are limited through a lack of knowledge to a few heads of classification and our development comes by increasing our genera or species. Coming more closely to our particular problem, we find that for many years, mankind has been divided into those who are sane and those who are insane, the latter class including all those people whose behavior was so far from established norms that they could not get along comfortably in the world by themselves.

To-day the mental defectives or feeble-minded are alluded to in England as cases of congenital insanity. However, of late, we have begun to draw a rather sharp line between insanity and mental

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defect or feeble-mindedness, the distinction being that feeble-mindedness is an arrest of development whereas insanity is a degenerative process, the victim not simply stopping where he is but losing a part of the mentality that he once had. In the early years of childhood, it is practically impossible to differentiate between these two. The result is that we are apt to call everything feeble-mindedness which occurs in the early years. And we have assigned as a cause of the feeble-mindedness whatever physical condition seems to be uppermost. For example, if we have a case of imbecility which also has hemiplegia, we classify it as a case of hemiplegic feeble-mindedness and assume, as a rule, that the cause which has resulted in the brain lesion producing the hemiplegia, has also caused the mental defect.

The thesis to be maintained in this paper is that this is not necessarily the case but rather that our whole problem will be simplified if we recognize some more heads of classification in this particular. To put the matter in another form, we may say that as the result of our study into the heredity of feeble-mindedness, we have come to the conclusion that the human family is divisible not only into the sane and insane, the healthy and diseased; but further, the sane and healthy group is subdivided possibly into many groups or *strains;* and that the mental capacity or possibilities varies widely in the different strains, but is fairly constant in each strain and is transmitted regularly, that is to say, so long as any given strain is kept pure, we will have the same mental capacity and possibilities generation after generation; that variations occur here as they do in the plant and lower animal world; that inbreeding and crossbreeding produce new combinations just as they do in plants or animals. To illustrate, we have only to call to mind almost any line of animal breeding. There is the genus horse, with various species, and within the species there are so-called strains. Every breeder knows that those strains will be transmitted and that they must be reckoned with in all attempts to breed horses for particular purposes or with particular characteristics. The same thing is true of dogs. No trainer would attempt to train a bull dog to retrieve or to point. Furthermore coming closer to our special problem, every trainer

knows that among the pointers, there are those that are easily and quickly trained to be high grade pointers; there are other strains that can never be trained to anything like the same efficiency.

The same thing seems to be true of the human race. There are strains that are capable of high mental development. These give us our geniuses or our brilliant leaders, or families with marked and valuable characteristics. Then there are strains with less capacity but still able to get along in the world and adapt themselves to their environment with fair success. These two groups are, of course, normal people. But when we go a step lower, we find a group of people whose capacity for development is so limited that they can never attain sufficient intelligence to get along in the world. Here we come to our group of feeble-minded and just as there are strains of varying degrees of intelligence which we call normal, so there are strains of varying degrees of defective intelligence, varying from those that are almost normal, almost able to take care of themselves, down to those who are so lacking in intelligence that they can do little more than procreate.

We are, for the time being, eliminating entirely all disease and abnormalities and fixing our attention upon what we call pure strains of feeble-mindedness. Furthermore, when this strain of pure feeble-mindedness is found uncomplicated as it often is, the mental condition is the chief peculiarity and the physical organism is oftentimes a remarkably perfect one, so that the strain is not recognized by any of its outward appearances, but only by those actions and movements which result from a less well-developed mentality. There can be found in institutions for the feeble-minded, persons of as fine physique and good health as can be found anywhere. This extends often even to details. For example: a dentist asserts that the finest set of teeth he has ever seen is in the mouth of an imbecile in an institution for feeble-minded.

Now it happens not unnaturally that these strains of defective mentality are liable to diseases of various kinds just as are so-called normal people. Whether the various diseases and accidents affect them in the same way as they affect people of normal intelligence, remains to be seen. It seems probable that in many cases the

effect is more serious and this accounts for the fact that the physical condition has in so many cases masked the hereditary factor and so our understanding of feeble-mindedness is usually much complicated by the presence of these diseased conditions. An illustration will make this clear.

Feeble-mindedness and epilepsy are often combined, constituting a complex that is very troublesome.

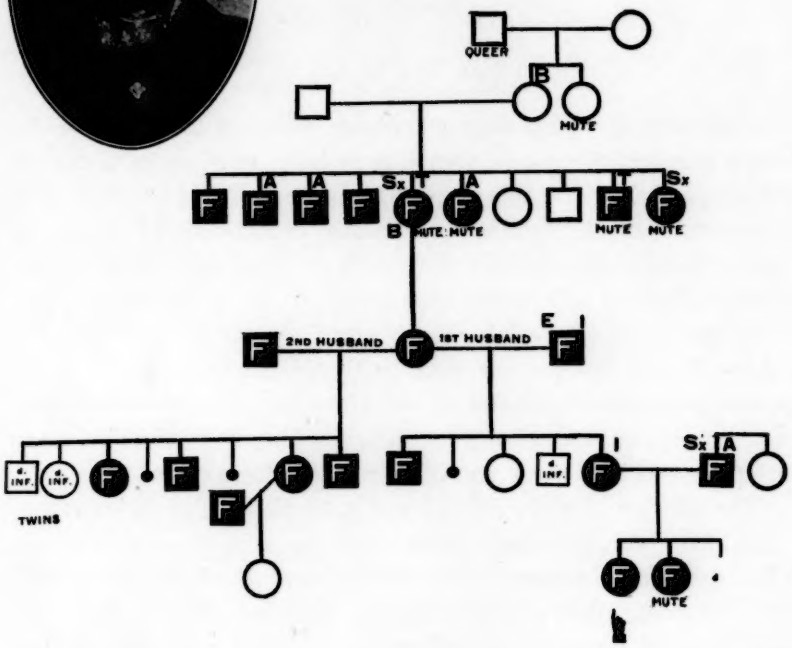
In the writer's opinion the problem would be much simplified by recognizing two groups: first those who belong to a strain of pure feeble-mindedness upon whom epilepsy has been grafted, and second a group of normal people suffering from epilepsy but in whom the epilepsy has produced an arrest of development and even set up a degenerative process. The clinical appearance of the two groups is much the same. The family history is needed to differentiate them.

Apparently also the Binet tests are useful on this line, those who are primarily feeble-minded testing in the same manner as other feeble-minded persons, that is to say, succeeding in the tests up to a certain definite point beyond which they cannot go, whereas those who are primarily normal but have deteriorated as a result of the epilepsy, show a scattering in their answers, that is to say, in some particulars, they show the intelligence of, for example, ten-year-old people while in others, they are only six, the degenerative processes set up by the epileptic attacks having destroyed certain mental processes and not others.

I shall not at this time go further into the question of the effects of the various diseases upon the different strains of mentality but shall content myself with showing you on the screen the portraits of ses of pure feeble-mindedness together with charts, showing that it is hereditary.

Note: The author showed a number of slides of perfectly normal-looking people but whose mentality ranged from that of two-year-old children up to that of ten-year-olds, although their physical age was in each case much greater. Each portrait was followed by a slide showing the hereditary character of the defect in several generations. One portrait with its accompanying family chart is here shown as an illustration.

Age 24 years.
Mental age, 10 years.



Squares are males: circles females. *F* = feeble-minded; *A* = alcoholic; *Sx* = sexually immoral; *T* = tuberculous; *B* = blind; *d inf* = died in infancy; *I* = insane; small black circle = miscarriage; *E* = epileptic. Hand points to child in the institution for the feeble-minded.

TRAINING SCHOOL FOR FEEBLE-MINDED CHILDREN,
VINELAND, N. J.

THE HEREDITY OF EPILEPSY ANALYZED BY THE MENDELIAN METHOD.

By DAVID FAIRCHILD WEEKS, M.D.

(*Read April 19, 1912.*)

Until recently it has been considered sufficient to determine the known number of epileptic ancestors or other relatives of a case of epilepsy, and then take this proportion as the index of heredity, with the natural result that the index increased as the study of the family was extended, resulting in a difference of from 20 to 75 per cent., as determined by different workers.

In our study of the inheritance of epilepsy at Skillman, we are endeavoring to analyze our data by the Mendelian method, which assumes that the inheritance of any character is not from the parents, grandparents, etc., but from the germ plasm out of which every fraternity and its parents and other relatives have arisen.

The relation of soma (body) and germ plasm is as follows:

1. If the body possesses a trait of the recessive to normality sort, it lacks the unit character upon which normal development depends, and it is *prima facie* evidence that the representative of that character is absent from its germ plasm, consequently such a person cannot transmit the character in question. The condition in the case when the determiner is absent may be called nulliplex.

2. If the body possesses a trait of the dominant to normality sort, it is evidence that the germ plasm has the corresponding determiner. But either one of two conditions is possible. (*a*) The determiner was derived from both parents, so that it is double in the germ plasm, and all the germ cells have the determiner; or else, (*b*) it came from one parent only, in which case it is single in the germ plasm, or simplex, and half of the germ cells have the determiner and half lack the determiner.

A moment's consideration will show that three kinds of somatic

and six kinds of germ plasm matings, disregarding sex, are possible. These latter matings, together with the sort of offspring they may be expected to yield, are as follows:

I. Nulliplex × Nulliplex = 100 per cent. Nulliplex.
II. Nulliplex × Simplex = 50 per cent. Nulliplex; 50 per cent. Simplex.
III. Simplex × Simplex = 25 per cent. Nulliplex; 50 per cent. Simplex; 25 per cent. Duplex.
IV. Nulliplex × Duplex = 100 per cent. Simplex.
V. Simplex × Duplex = 50 per cent. Duplex; 50 per cent. Simplex.
VI. Duplex × Duplex = 100 per cent. Duplex.

Practically, it is not always easy to distinguish the simplex from

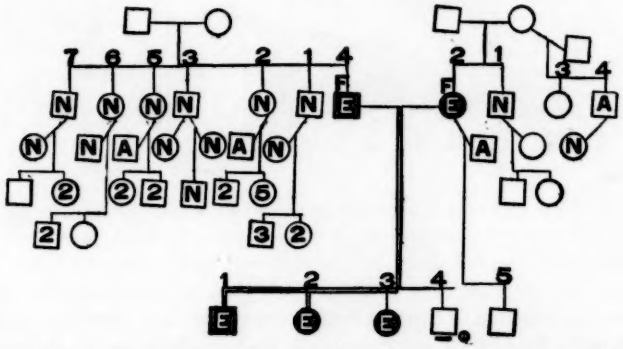


FIG. 1. In the above chart both of the parents are epileptic. There were four children from this mating, three of them were epileptic and the fourth, after the chart was made, was discovered to be feeble-minded. This case is of the type nulliplex × nulliplex. *E*, epileptic; *F*, feeble-minded; *A*, alcoholic; *N*, normal. Case 3,667.

the duplex condition, although frequently a simplex condition is indicated by an intermediate mental status.

The new method of obtaining the material is largely responsible for any advance which has been made in the study of the inheritance of epilepsy.

The field workers visit the parents and other relatives of the patient and interview them in their homes, where they are at ease, and free from restraining influences, as would not be possible elsewhere. The family physician, clergyman, school teacher and other interested persons are also interviewed, for the purpose of securing an accurate account of the mental status, environmental conditions,

diseases, and causes of death, if dead, of as many relatives of the patient as possible.

The data thus obtained are recorded and tabulated on a heredity card, in such a manner that future additions and corrections can be easily made, without destroying or detracting from the work done to date.

The history and chart are filed with the case records. A cross index of place, name and trait help to locate the defective strain by

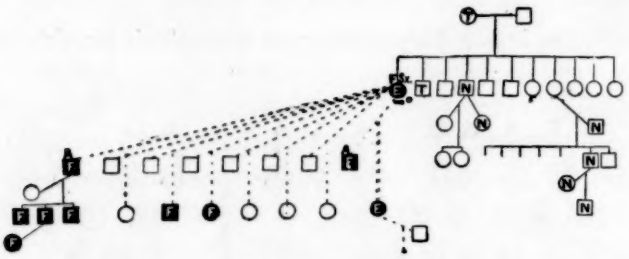


FIG. 2. This chart is a good illustration of the source of a large number of almshouse inmates. The central figure is an epileptic woman who has had seven illegitimate children all by different men; three of these died in infancy and the remainder are defective. This woman, who has spent the greater part of her life in the almshouse, was taken from there to keep house in a miserable hut for a feeble-minded man, one of whose feeble-minded sons married the feeble-minded sister of an epileptic man who is a patient at the New Jersey State Village. Her daughter, who is the patient, had one illegitimate child before she was cared for by the proper authorities. *E*, epileptic; *F*, feeble-minded; *A*, alcoholic; *N*, normal; *Sx*, sex offender; — — — —, illegal union. Case 586.

family name, defect and locality. An index of the names of individuals charted facilitates the tracing of families from one pedigree to another. A register is kept by counties, showing as far as possible the locality from which the individual comes; the age; the institution or other care received, no care, etc., of every epileptic known to us in the state. A reference index of relatives living in different counties, states or foreign countries, together with the name and location of any institutions in which they have been treated, is also kept.

It will be seen at a glance that data thus obtained have proved to be much more significant and trustworthy than the familiar family

history obtained from the patient or his guardian at the time of his admission to the institution, and we are convinced, justifies careful study, which is more than can be said of the former history.

This study is based on 397 separate pedigrees of this number, however, seven came from the same fraternity; in another three are from the same fraternity and their mother is also a patient in the institution. In four other cases, two patients are from the same fraternity, and besides these, four others have been found to be

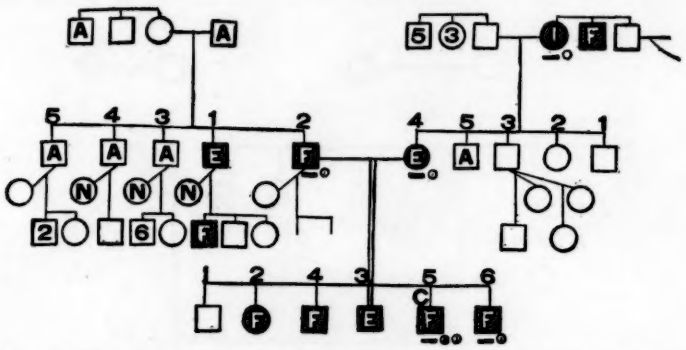


FIG. 3. The central mating in this case is that of a feeble-minded man and an epileptic woman. The man had an epileptic brother, who in turn had a feeble-minded son, while the woman came from an insane mother and had a feeble-minded uncle. There were six children; the first died in infancy, the second and fourth were feeble-minded, the third was epileptic, the fifth is a feeble-minded boy, who is at The State Home for Boys, while the last is also feeble-minded, and he is cared for at a Children's Industrial Home. The mother and father are dependent on the town for support, the mother's mother died in the State Hospital for the Insane. This mating is of the type nulliplex × nulliplex. *E*, epileptic; *F*, feeble-minded; *I*, insane; *A*, alcoholic; *N*, normal. Case 4,369.

related, thus connecting their pedigree with others already acquired, so that only 381 different families are involved in our study.

The frequency with which the same name occurs on many of the charts indicates that there is little doubt but that future study will determine their relationship and show some of these to be of the same blood.

The total number of epileptics recorded on the charts was 756, which was 3 per cent. of the total chart population of 21,558, or 9 per cent. of the 8,698 classified individuals.

In analyzing our data, we have classed it under the six kinds of matings, as follows:

NULLIPLEX × NULLIPLEX.

There are twenty-seven fraternities in which both parents are either epileptic or feeble-minded; 16 of these matings are principal matings and 11 secondary matings.

In three of the matings both of the parents were epileptic. Of the 28 conceptions, two were stillbirths, 3 miscarriages, 3 died before

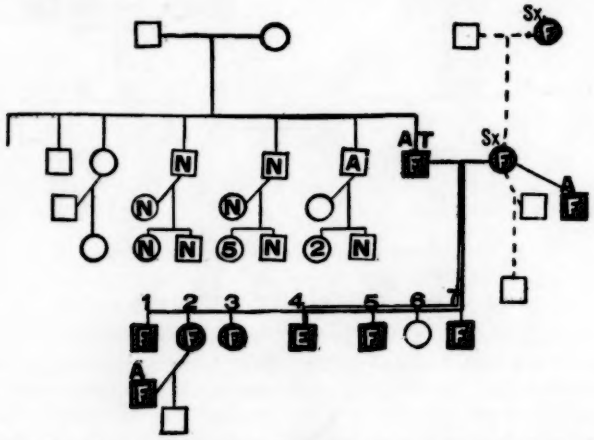


FIG. 4. This chart shows the offspring in a case where both the father and mother were feeble-minded; the father was alcoholic and died of tuberculosis, while the mother was sexually immoral and was the illegitimate child of a feeble-minded woman. There were seven children; one, the sixth, is thought to be by a different father; of the others five are feeble-minded and one is epileptic. After the father's death the mother married a feeble-minded man, who is the younger brother of her daughter's feeble-minded husband. *E*, epileptic; *F*, feeble-minded; *A*, alcoholic; *T*, tubercular; *Sx*, sexually immoral; *N*, normal; — — — — —, illegal union. Case 3,037.

two years of age, and one (an infant) is too young for classification, leaving 19 about whom something definite is known. Of these, 8 were epileptic, 3 feeble-minded, and 8, who came from parents who developed epilepsy late in life, were tainted. (Fig. 1.)

In fifteen fraternities in which one parent is epileptic and the other feeble-minded, there were 81 conceptions; 7 were too young

to be classified, and 19 died before 14 years of age. Of the 55 classified, 29 were epileptic, 26 feeble-minded and 1 insane. (Figs. 2 and 3.)

In nine fraternities in which both parents were feeble-minded, there were 56 conceptions. Of these, 4 died before two years of age, 14 were too young for classification. Of the other 38 of whom something definite is known, 7 were epileptic, 28 feeble-minded and 2 drunkards, who may or may not have been feeble-minded. (Fig. 4.)

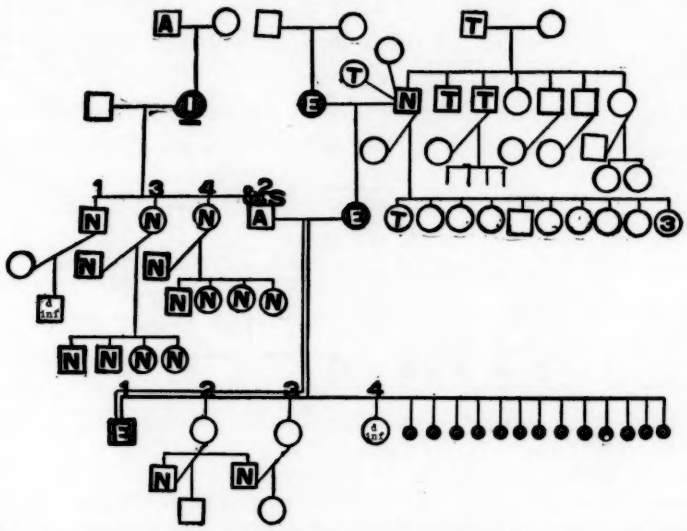


FIG. 5. In this history the mother is epileptic and descended from an epileptic mother; the father, who is alcoholic, is also syphilitic; his mother was insane. The first child is epileptic, the next two are neurotic, the last died before two years of age, and following this there were thirteen miscarriages. In contrast to the central mating this chart shows two normal matings; in these cases all of the children are normal. *E*, epileptic; *F*, feeble-minded; *S*, syphilitic; *I*, insane; *Sx*, sex offender; *N*, normal; *A*, alcoholic; *d inf*, died in infancy. Case 1,772.

These matings are of the type nulliplex × nulliplex, and seem to justify the expectation that all children from these unions will lack the determiner for normality.

In five fraternities where one parent is insane and the other epileptic or feeble-minded, there were 29 conceptions; 5 died before 14 years of age, 2 unknown. Of the 22 available for study, 2 are

epileptics, 4 feeble-minded, 1 insane, 8 tainted and 7 seemingly normal. These latter came from two fraternities, where in one case the father's insanity seemed to be traumatic and in the other alcoholic.

Nulliplex × Simplex

Under this classification we have grouped separately those fraternities in which one parent was alcoholic.

In the consideration of the fraternities where one of the parents is epileptic or feeble-minded, and the other alcoholic, we have classed as alcoholic all of those parents who are habitually hard drinkers, or who go on frequent sprees.

In thirty-five fraternities there were 226 conceptions; 21 are yet too young to be classified, 79 died before 14 years of age (which is

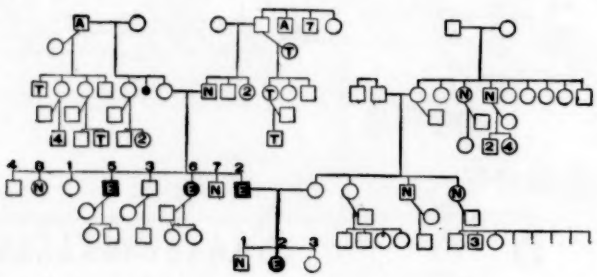


Fig. 6. In this case the father is epileptic and the mother is neurotic. The father had an epileptic brother and an epileptic sister. There were three children; the first was normal, the second epileptic and the third too young as yet for classification. This is an illustration of the nulliplex × simplex type of mating. *E*, epileptic; *F*, feeble-minded; *N*, normal; *T*, tubercular; *A*, alcoholic. Case, 3,402.

35 per cent., a high death rate). Of the 126 remaining, 39 are epileptic, 38 feeble-minded, and 28 showing some other taint (1 insane, 3 migrainous, 16 neurotic, 5 alcoholic and 3 sexually immoral), with only 21 apparently normal.

In these matings of the type nulliplex × simplex, we should expect 50 per cent. nulliplex, that is, epileptic or feeble-minded and 50 per cent. simplex, that is, apparently normal or showing only some slight defect. We have, however, 61 per cent. nulliplex, 39 per cent. simplex, the increase over the expectation being probably

due to the fact that the alcoholic parent was also mentally defective, or that the alcoholism may, through poisoning the germ cells, be a contributing cause of epilepsy. (Fig. 5.)

In addition to the above, there were six matings where one parent was insane and the other alcoholic. Of the 29 conceptions, 9 died in infancy, 6 are unclassified, leaving 14 of whom something definite is known. Of these, 7 were epileptic, 3 feeble-minded, 2 neurotic and 2 apparently normal.

In considering the fraternities in which one of the parents is either epileptic or feeble-minded, and the other "tainted," we have classified as "tainted," the migrainous, neurotic and those who are mentally weak.

There were twenty-five fraternities with 161 conceptions; 60 died before 14 years of age, 24 are too young for classification, leaving 77 for study. Of these, 27 or 35 per cent. are epileptic, 9, or 11 per cent., feeble-minded, 24 showing some slight nervous or mental weakness, and 17 normal.

In these matings we should expect 50 per cent. nulliplex and 50 per cent. simplex. We obtained 47 per cent. mentally defective and 53 per cent. apparently normal, or showing some slight weakness, which is a very close fitting to the expectation. The discrepancy may be accounted for by the possible classification as simplex of individuals who are in reality nulliplex. (Fig. 6.)

In six matings of an insane parent with a "tainted" one, there were 45 conceptions; 10 died in infancy, 9 are unclassified. Of the 26 others, 9 were epileptic, none feeble-minded, 8 tainted, with 9 apparently normal.

Nulliplex × Duplex.

Under this classification we have tried to place the fraternities of which one parent was epileptic or feeble-minded, and the other reported normal.

In thirty-eight fraternities with 223 conceptions; 62 died in infancy, 36 are too young for classification. Of the 125 others, 39 or 32 per cent. are epileptics, 14 or 11 per cent. are feeble-minded,

and 26 or 21 per cent. are neurotic, while 46 or 39 per cent. were apparently normal.

This fitting confirms very closely with what might be expected from the type simplex by nulliplex, indicating that the parents who have been classified as duplex (normal) are really simplex, in that half their germ-cells have and half lack the determiner for normality.

Twenty of the normal parents had ancestors who showed some mental or nervous weakness, this would justify their classification as simplex. In 26 cases little is known about the ancestors of the normal parent. The available information about three would

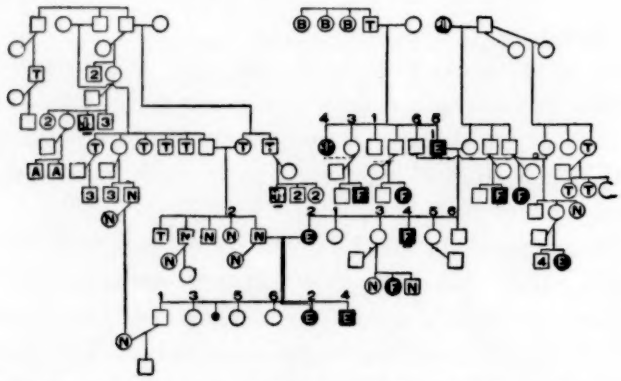


FIG. 7. The central mating in this pedigree is that of an epileptic woman, who descended from an epileptic father and has several defective relatives, and a normal man, who comes from normal ancestors. There were six children, two epileptic, and four neurotic. *E*, epileptic; *F*, feeble-minded; *I*, insane; *T*, tubercular; *B*, blind; *N*, normal. Case 2,207.

indicate that they are mentally normal. Judging from their offspring, we believe that subsequent data will show that these apparently mentally normal parents descended from tainted ancestors. (Fig. 7.)

Eleven matings of an insane and normal parent, resulted in 50 conceptions; 10 died before 14 years of age, 7 are too young for classification. Of the 33 remaining, 12 or 36 per cent. were epileptic, 2 or 6 per cent. feeble-minded, 18 or 55 per cent. seemingly normal, while 1 or 3 per cent. was neurotic.

Simplex × Simplex.

Under this type of matings we have grouped those fraternities coming from matings where neither parent can be classified as normal, or called mentally deficient, but showing some mental or nervous weakness.

There were eighty-four matings of this type, with a total of 540 conceptions. 152 died in infancy, with 52 unclassified. Of the 336 others, 97 were epileptic and 17 feeble-minded, in other words, 114, or 35 per cent., were nulliplex, an excess of 10 per cent. over the expected 25 per cent. (Fig. 8.)

In these matings there was an excess of epileptic and feeble-

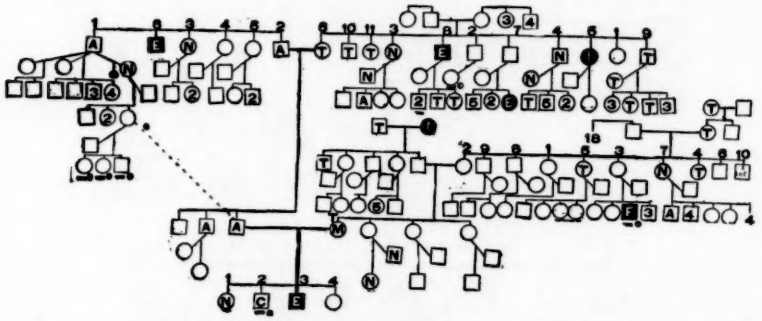


Fig. 8. In the central mating, the alcoholic, unchaste man who comes from a "tainted" strain, married a migrainous woman. There were four children: the first is normal, the second, criminalistic and has been an inmate in the State Reform School, the third is an epileptic, and the last is neurotic. This is an illustration of the simplex × simplex type of mating. *E*, epileptic; *F*, feeble-minded; *I*, insane; *A*, alcoholic; *T*, tubercular; *M*, migrainous; *N*, normal. Case 2,029.

minded beyond the expectations, which would seem to indicate that some of these tainted conditions are more closely allied with the cause of epilepsy and feeble-mindedness than has so far been recognized. The fact that there were more than five times as many epileptics as feeble-minded persons, tends to show that the neurotic and otherwise tainted conditions are more closely allied with epilepsy than with feeble-mindedness. It is a significant fact that out of the 84 matings, in four of them both parents were migrainous, while in 23 one parent was migrainous, and 14 of these were mated

to alcoholics. Of the remaining 57 matings, in 7 both parents were alcoholic, and in 30 matings one parent was alcoholic, making a total of 44 (14 + 30) where one parent was alcoholic.

Simplex × Duplex.

Under this classification we have attempted to analyze those fraternities in which one parent was "tainted," although not epileptic or feeble-minded, and the other one normal.

In one hundred and twenty-seven fraternities there were 790

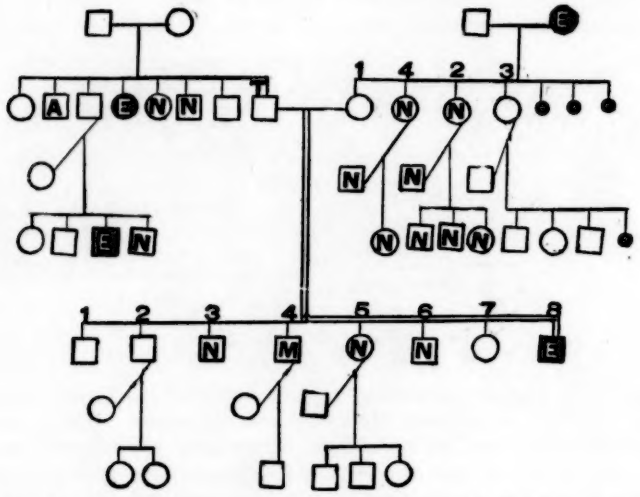


Fig. 9. In this case a neurotic woman, descended from an epileptic woman, married a man who was mentally normal, but who had an epileptic sister. The first child died at the age of seven, the second has defective speech, the third, fifth and sixth were normal, the fourth was migrainous, the seventh had St. Vitus dance and the last is an epileptic. *E*, epileptic; *F*, feeble-minded; *M*, migrainous; *A*, alcoholic; *N*, normal; *T*, tubercular. Case 2,673.

conceptions. Of the 477 classified as normal, mentally deficient or tainted; 130, or 27 per cent., are epileptic, 15, or 3 per cent., feeble-minded. (Fig. 9.)

At least 74 of these reported normal parents have been found by the field worker to have tainted heredity, so that these matings

are of the type simplex × simplex, and the findings would seem to indicate that all the matings are of this type.

Duplex × Duplex.

Under this type of mating we have grouped all those fraternities in which both parents are recorded as normal.

In the study of seventy-six fraternities there were 441 conceptions; 62 are too young for classification or entirely unknown, 103

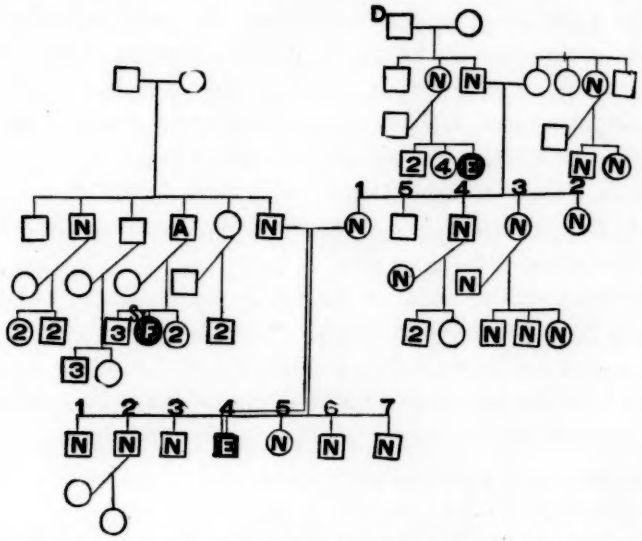


Fig. 10. This chart shows the mating of two normals. They each have defective relatives. Of the seven children the fourth is an epileptic and the others seem to be normal. *E*, epileptic; *F*, feeble-minded; *A*, alcoholic; *N*, normal. Case 2,983.

died before the age of 14 years. Of the 276 others, 172 are normal, 75, or 27 per cent., are epileptic and 3, or 1 per cent., feeble-minded, with 1 insane and 25 tainted, that is, 197 (172 + 25), or 71 per cent., are duplex or simplex in character. (Fig. 10.)

The results show that these normal parents are not duplex, but simplex, and a study of their ancestors justifies this conclusion, for in at least 45 of the matings, one or both of the parents descended from tainted ancestors.

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All the available facts point toward the conclusion that the various common types of epileptics seen in institutions lack some element necessary for complete mental development, which is also true of the feeble-minded.

Two epileptic parents produce only defective offspring, when both parents are either epileptic or feeble-minded, their offspring are also defective, the defect taking the form of epilepsy, feeble-mindedness or some other neuropathic condition. This is also true of feeble-minded parents.

In the light of our present knowledge, the results obtained from the study of our data do not justify the classification of the reported normal parents of epileptics as duplex. We are forced to the belief that their germ plasm is simplex, and feel confident that more complete data would show the taint in their ancestors.

Our data seem to support the belief that alcohol is a cause of defect, in that more children of alcoholic parents are defective than where alcoholism is not a factor.

That there are more than five times as many epileptics as feeble-minded persons in those fraternities coming from matings where neither parent can be classed as normal, or called mentally defective, seems to indicate that neurotic and otherwise tainted conditions are more closely related to epilepsy than to feeble-mindedness.

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IS THE CONTROL OF EMBRYONIC DEVELOPMENT A PRACTICAL PROBLEM?

By CHARLES R. STOCKARD.

(*Read April 19, 1912.*)

Under favorable natural conditions two normal parents should, and usually do, produce a vigorous normal offspring. When, however, the conditions of development are modified or if in the second place the parents are not entirely normal the offspring is usually more or less defective. I shall attempt to show that the proper development of the offspring is dependent upon two main factors, first the physical qualities of the parental germ cells, and second the environment in which the embryo develops.

One is at first sight apt to think that deformities and defects are rare among men and other animals; but closer observation will show that the really structurally perfect individual is rather exceptional. Gross anatomical defects or monstrosities are frequently found among all animals, while lesser defects of minor importance are to be observed in a majority of individuals. These defects often cause no inconvenience, and indeed, we may be ignorant of their presence, since they are generally internal. Yet many apparently normal individuals sooner or later suffer or may actually die from some hidden developmental imperfection. The well-known congenital defects of the heart and other parts of the vascular system, digestive tract, etc., as well as the numerous developmental arrests in various parts of the body constantly remind the observer of the great loss in ability and energy that the race suffers as a result of faulty development.

These defects in construction must be considered a disease which causes the death of about 23 per cent. of the human race before or shortly after the time of birth (Sullivan's studies and French statistics), and handicaps a certain proportion of the survivors through-

out their lives. We carefully study and use all known precautions to protect ourselves against post-natal infections and diseases, and much interest and time is given to combating the causes, yet little is said and scarcely anything done towards a control of development, or the hygienic protection of the developing individual.

This is really a morphological problem and is as truly a part of the fight against disease as is the treatment of abnormal physiological processes. It is not all of morphology to describe and study the detail of bodily structure, but its important task is to understand and analyze that structure, and if possible control and regulate its formation: and thus, if properly developed its goal is to relieve the race of its great structural disease—a disease which affects more individuals than any other one malady of man.

To most persons the above task seems at first thought a futile undertaking, and any one suggesting such control or preventive treatment might be interpreted as indulging in fanciful speculation. Yet the data available from the studies of defective persons in different countries of the world, and the experimental evidence furnished by work on lower animals makes the correction or prevention of developmental defects seem even today a problem to be practically handled to a slight degree at least.

To proceed as with any other disease, we must first ascertain the cause of these conditions, as the possibility of a cure depends upon the nature of the cause.

Are monstrosities and defective development due to some innate change within the germ cells of the parent, thus being incurable, as many former workers would have us believe? Or, are they due to changes produced in the germ cells by the action of some unusual condition in the body of either the male or female parent, or finally may they not be due to an unusual environment acting upon the developing embryo itself? In both of the latter cases the conditions are open to regulation or control. These questions may only be solved experimentally and the experiments have proven that the great majority of monsters are due to the action of unusual conditions upon either the parental germ cells or the developing embryo. There may be some changes of form or variations in animals which are due to

innate changes in the germ-plasm but even these when fully understood may possibly be shown to result indirectly from some change in the chemical surroundings.

First to consider the modifications induced in the developing egg or embryo by a strange chemical environment. It has been found for the eggs of a number of animals that develop normally in sea-water that when certain chemicals are added to their environment they develop into various unusual forms.

I experimented for several years on fish's eggs and found that on adding any one of a large series of salts to the sea-water that the eggs developed abnormally and gave rise to a great number of monstrous individuals. The types of the monstrosities were variable, and the same kind of monster often resulted from different treatments. This was to be expected, but the important problem was to produce some definite type of monster in great numbers with any given treatment. This I finally succeeded in doing and in some experiments got as many as 90 per cent. typical cyclopean or monophthalmic monsters. These types of monsters first occurred in solutions of $MgCl_2$ in sea-water. In such solutions as many as 50 in 100 eggs formed one-eyed cyclopean embryos. Since Mg has the power to inhibit activity in animals and so acts as an anæsthetic I determined to try the action of a number of such substances on the developing eggs to ascertain whether they might also inhibit the lateral migration of eye parts. Alcohol, ether, chloroform, chloreton, etc., were employed and cyclopean monsters resulted from eggs developing in all of these substances. Alcohol gave the most decided effects and inhibited the normal production of eyes in almost all cases. All of these anæsthetics act more particularly upon the central nervous system of the adult and it is important to find that the development of the nervous system is also especially affected by them. In alcohol solutions the embryos showed almost every gross abnormality of the brain which is known to occur, and the spinal cord was often defective.

I have repeated the experiments of Féré with hen's eggs and find that when these eggs are exposed to fumes of alcohol many abnormal chicks result. When hen's eggs are placed in closed dishes over

evaporating 95 per cent. alcohol enough of the fumes penetrate the shell and enter the contents of the egg to cause the developing chick to form abnormally.

McClendon has lately found that an excess of CO_2 and other substances also cause cyclopia and brain abnormalities. Many other workers have shown the effects of the environment on the developing egg.

It is, therefore, proven that the experimenter has the power to take an egg which would normally give rise to a perfect animal and by proper treatment he may cause it to form a typically abnormal individual. The monster may in many cases be able to survive and move about. No one can question that in these experiments the unfavorable environment modifies the form of the resulting individual.

Does this also occur in embryos developing in the mother's body? Children are born which exhibit the same types of deformities as those described above. Syphilitic mothers usually abort or give birth to abnormal children and there is much evidence to indicate that an alcoholic mother is more apt to produce an abnormal child than is a non-alcoholic mother.

Tubal pregnancies are common among women with venereal diseases and in such cases the embryo must necessarily develop under abnormal environment, having a poor surface for placental attachment in a region not adapted to the conditions of pregnancy. The conditions for embryonic nutrition are poor. Mall has found that while only 7 per cent. of uterine pregnancies in his records contained pathological embryos, that 96 per cent. of the embryos in tubal pregnancies were pathological, only 2 in 46 specimens being normal. This is strongly indicative of an abnormal environment as the cause of abnormalities. If these monsters were due to inherent tendencies in the germ cells one should not expect more abnormal tubal than uterine embryos.

Among lower mammals it has been shown that dogs fed on alcohol produce deformed and otherwise defective pups. I am now conducting a series of experiments with guinea pigs which show that a female treated with alcohol during her pregnancy will often

abort or produce defective young, while the control animals are giving birth to normal young. Many more cases could be cited if time permitted.

Experiments on lower animals, therefore, show and human statistics seem to indicate that the cause of structural disease is often an abnormal developmental environment. To prevent such a disease the developmental conditions must be controlled and rendered as nearly normal as possible.

The second consideration is whether abnormal chemical environment may act on the parental germ cells in such a manner as to cause them to change and become incapable of giving rise to a normal individual. It is well known that certain disease toxins such as that of syphilis and substances such as alcohol and lead effect various body tissues so as to render them unfit for normal physiological activity. It is, therefore, only logical to suppose that the same or similar substances may effect the germ cells and so derange their chemical constitutions as to cause them to give rise to offspring of peculiar structure and qualities.

Bertholet has found that alcohol has a particular affinity for the reproductive glands just as it does for the nervous system. In examining the structure of the testicles from a large number of chronic alcoholics it was shown that spermatozoa were absent entirely or degenerate in form (azoospermy) in a majority of the cases. It is doubtless true that the ability of the spermatozoa to accomplish normal fertilization would be affected long before any definite structural change could be observed.

The crucial case is the treatment of the male in such a way as to render his spermatozoa unable to produce a normal development when combined with a healthy egg from a normal female. In this case the action must of necessity be on the germ cell only and not on both the egg and embryo as it might be in treating a female mammal.

It must be recognized that an individual owes its structure and character to the peculiar chemical constitution of the germ cells from which it arises. The germ cells of two species of animals are

probably as different chemically as the animals are morphologically. Therefore, if the chemical nature of the germ cells is disturbed or injured by the action of poisons in the animal's blood they will probably show this injury in the type of individual to which they give rise.

Constantine Paul long ago found in studying 88 cases of pregnancy among women lead workers that 71 resulted in abortion, premature labour, or stillbirth while only 17 children were born alive and of these five died within the first year. Several of these women later produced healthy children after leaving this work. (This indicates that when the cause is known for defective development the cure may often be established by its removal.) Lead not only effects the developing fœtus but also acts directly upon the germ cells as is shown in the case of men working in lead while their wives were not exposed to the poison. Many of the offspring from such fathers are aborted and the children born are epileptic, feeble-minded or generally defective.

To return to the results furnished by the guninea pig experiments referred to above—I have chosen healthy individuals and treated them daily with the fumes of 95 per cent. alcohol to about the point of intoxication. Feeding alcohol and giving it by stomach tube was first tried, both of these methods were unsatisfactory as the guinea pigs did not take alcoholic food in sufficient quantity and the stomach tube disturbed the animals to such a degree that I feared the experimental result might be vitiated even though it could be partially controlled. The inhalation method is perfectly satisfactory; the animals are placed in a copper tank having a screen floor which holds them above the evaporating alcohol. The alcohol is breathed directly into the lungs and affects the animals readily, in much the same manner as weak treatments of ether or chloroform would. The animals are thus put into a condition of chronic alcoholism, being almost intoxicated six times per week. Many of these guinea pigs have been killed and their lungs, liver and other organs examined and found to be perfectly normal so far as their appearance goes. The conjunctiva over the eyes is very often affected by the fumes, during the beginning of the treatment the eyes often

become white, this is transitory in most instances and the eyes finally clear again and remain in a normal condition from then on. Most of the specimens have fattened under the alcohol treatment.

The matings have been made in such a fashion as to test several questions. First, alcoholic males are mated with normal females, paternal influence, the crucial test for the effect upon the germ cells. Second, alcoholic females are paired with normal males, the maternal influence plus the direct action on the developing embryo. Lastly, alcoholic males and females are paired.

The results of 40 such matings are shown in Table I. The decided effects of the alcoholic treatment are seen when the records are compared with those of the normal guinea pigs.

TABLE I.

MATINGS OF ALCOHOLIZED GUINEA-PIGS.

Condition of Animal.	Number of Matings.	No Result or Early Abortion.	Stillborn Litters.	Number of Stillborn.	Living Litters.	Early Deaths.	Surviving Young.
Alc. male × nor. female	24	14	5	8	5	5	5 1 + 4
Nor. male × alc. female.....	2	1	0	0	1	0	1 Preg. 2 in utero, 1 deformed.
Alc. male × alc. female.....	14	10	3	5	1	1 died 6th day	0
Summary.....	40	25	8	13	7	6	6 6 in 25
Nor. male × nor. female. *Control.*	8	0	0	0	8	0	15 15 in 15

In the 24 cases in which normal females were mated with alcoholic males, 14 gave negative results. Some of these probably aborted early as the parents were all fertile and the female is apt to eat the young before they have been observed when they are born prematurely. Five of the matings gave stillborn young, in some cases they were born a little before term. Litters were born alive but the young died soon after showing many nervous symptoms, such

as epileptic-like seizures, and all died in convulsions. Only two litters consisted of normal offspring and these young, five in all, seem healthy though unusually small. It is thus seen that in 24 matings of *normal* females with alcoholic males only two gave normal results. Whereas in the control animals all matings have resulted in the production of normal offspring.

Only two matings were made between normal males and alcoholic females. One of these gave no result or was possibly aborted very early and lost, while the other mating produced one female offspring that lived to become pregnant by an alcoholic male. This last mentioned female was killed by accident, two embryos were found *in utero* one of which was deformed.

Fourteen matings were made between alcoholic males and females. Ten gave no result or aborted very early and were eaten, while four cases showed the following records. One young was born weak and died in convulsions on the sixth day after birth. Two cases of premature births of dead young. One female had young *in utero* when killed.

TABLE II.

SUCCESSIVE MATINGS OF TEN FEMALES.

Animal.	1st Mating.	2d Mating.	3d Mating.	4th Mating.
No. 10 alc.	Alc. male 4 = 1, young died 6th day.	Alc. male 4 = 0.	Alc. male 6 = 0.	Alc. male 4 = 1, embryo in utero 2nd week.
No. 12 alc.	Alc. male 5 = 0.	Alc. male 5 = 0.	Alc. male 4 = 0.	
No. 11 alc.	Alc. male 6 = 0.	Alc. male 6 = 0.	Alc. male 5 = 2, premature, still-born.	Alc. male 4 = 0.
No. 13 nor.	Alc. male 5 = 1, stillborn.	Alc. male 5 = 0.	Alc. male 4 = 0.	
No. 17 nor.	Eth. male 1 = 0.	Eth. male 1 = 0.		
No. 18 nor.	Alc. male 5 = 0.	Alc. male 5 = 0.	Alc. male 6 = 0.	
No. 7 nor.	Eth. male 2 = 2, premature, stillborn.	Eth. male 2 = 0.	Eth. male 2 = 0.	
No. 14 nor.	Eth. male 3 = 0.	Eth. male 3 = 0.		
No. 19 nor.	Alc. male 4 = 0.	Alc. male 6 = 1, stillborn.	Alc. male 6 = 0.	Alc. male 5 = 4, small but active.
No. 15 nor.	Alc. male 6 = 2, stillborn.	Alc. male 5 = 0.	Alc. male 5 = 2, died 4th week.	Nor. male = 1, normal young.

These results stand in marked contrast to the records of the control, which show all normal conceptions and normal offspring.

The second table shows the results of successive matings in ten of the females. The varying success of the conceptions in the same individual are striking.

Nice has quite recently recorded a similar series of experiments with alcohol on mice. Alcohol was given to the mice in their food. Nice finds that while there was a certain fatality among the offspring from alcoholic parents as compared with those from normal parents, where there was no fatality, yet nevertheless the offspring of alcoholic parents actually grew faster than those from the control. This may indicate that alcohol is not equally poisonous in its effects upon all animals, as might really be expected. The germ cells of mice may be more or less immune to the action of alcohol. It is well known that the action of alcohol is different in its effects on individuals from different human families. Some alcoholics show chiefly nervous disorders, hallucinations, delirium, etc., while others may have no nervous symptoms but exhibit various derangements of the digestive glands, kidneys, etc., or may have a fatty degeneration of almost all organs.

Finally it may be concluded that the experimental evidence goes to show that the development of an offspring may be modified by either treating the parents so as to affect their germ cells or by subjecting the developing embryo itself to unusual or injurious conditions.

The causes of many congenital defects are therefore known. It is possible to control embryonic development to such an extent as to produce abnormal structures. May not the proposition be reversed and unfavorable environments be treated in such a manner as to render them favorable to normal development? Diseased mothers may in some cases, at least, be made fit for the function of reproduction.

The regulation of structural disease becomes then a problem of morphology and hygiene. It is most important, and must precede,

or go before, the selective mating of human beings or the eugenics movement. The most intellectual will rarely submit to direction in choosing a mate, yet every productive pair will welcome any possible means of improving the quality of their offspring.

While preventive measures are being used to protect the post-natal life of the individual, why not guard as far as possible its pre-natal development?

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AN AVIAN TUMOR IN ITS RELATION TO THE TUMOR PROBLEM.

By PEYTON ROUS, M.D.

(*Read April 19, 1912.*)

The behavior of malignant tumors has stimulated many hypotheses as regards their causation and yet in some of its phases has appeared to disprove all. The suggestions afforded by other normal or pathological growth processes lead in numerous and diverse directions. Today, despite an immense accumulation of data, the solution of the tumor problem waits upon fresh findings; and to foretell the line of research which will yield these findings has not seemed possible.

The successful transmission of neoplasms of the lower mammals, a few years ago, seemed at first to carry with it the immediate solution of the problem. But this did not prove to be the case. In order to obtain a tumor-strain for investigation, an animal with a "spontaneous" tumor was required; and the transmission of the growth was soon found to involve a transplantation process,—as genuine a transplantation as that of skin or other normal tissue. A new tumor in the strict sense was not thus engendered, but to a portion of the old another host was given. All efforts to separate out a cause for the neoplasm or to transmit it by other means than by graft of the living neoplastic cells were unsuccessful. The consistently negative results of such work, together with the general behavior of the transplanted neoplasms, have led many investigators to forego the idea of an extrinsic cause for malignant tumors in general and to attribute them to some inherent cell-perversion, or else to a cell-derangement precipitated by factors temporarily active. But it may be pointed out that the basis for such a conclusion so far as it rests upon experiment, rests upon work with the tumors of few species, Those of the rat and mouse have been employed almost exclusively.

The findings here to be presented were obtained in the study of a malignant tumor of the chicken, which closely resembles in its general characters the mammalian neoplasms, including those of man. That such growths exist has been generally recognized; and their status as true tumors has been established. But like the neoplasms of other birds, of reptiles, amphibians, and many mammals, they have remained almost unutilized for research. Our tumor of the fowl proved transplantable and has thus far been observed in several hundred chickens. It is a connective-tissue growth, a spindle-celled sarcoma. From its tissue there has been isolated a causal agent, ultramicroscopic in some, perhaps in all, of its forms, and undoubtedly a living organism. Though the agent gives rise to the sarcoma, and accompanies the growth, it does not take any obvious share in the disease phenomena. These are referable to the behavior of the neoplastic cells, a point now to be illustrated.

The original sarcoma arose in a young fowl of pedigreed, pure-blood stock, and its transplantation was successful only in this fowl's blood-relations. A similar peculiarity has been often observed on the transplantation of normal tissues, but it has not been noted in association with the transmission of diseases caused by a parasite. After propagation in several successive hosts, the sarcoma became less precise in its demands and could be transplanted to non-related fowls of the same variety. But like certain delicate tumors of mammals it was for a long time transmissible only within the limits of this variety, and at the latest test still grew most readily in such hosts. All attempts to transmit it to animals of other species have failed.

A transplantation of neoplastic tissue is involved in the growth's transmission under ordinary circumstances, and only by special means has it been shown to be unnecessary. Ordinarily when bits of the sarcoma are placed in a new and susceptible host they survive, are vascularized, and proliferating, form a new tumor. The multiplication of the implanted cells obviously suffices to produce the neoplasm. In the histological pictures there is no indication that the elements of the host ever become incorporated as true neoplastic tissue. No tumor arises in hosts so unfavorable to the engrafted tissue that it dies.

The growth consists of spindle-shaped cells supported by a scanty, vascular framework; and the sole differentiation which these cells undergo is to an attenuated form with the production of a few intercellular fibrils. In the disposition of the cell-strands and bundles there is no suggestion of focal arrangement such as frequently indicates, in the case of the granulomata, the presence and position of an exciting cause; while at the growth's borders a cellular reaction is practically absent. The tumor elements multiply rapidly by mitosis and amitosis, and the neoplasm grows, not only by expansive enlargement but also through an active invasion and replacement of the normal structures by tumor cells. In the course of the invasion tumor cells frequently penetrate the walls of blood or lymph-vessels, and are freed in the circulation. By their transportation, lodgment and growth secondary sarcomata are caused at points distant from the primary mass. This important characteristic of tumors in general has been placed beyond doubt as regards the avian growth by means of direct experimentation. The host, which at first seems unaffected by the tumor, emaciates as the growth increases in size, and, if it escapes intercurrent processes eventually dies in coma.

The conditions which determine the success or failure of the sarcoma when transplanted to a new individual are in general referable, as are those of its behavior and dissemination, to the sarcoma cells as such. The influence of variety of the host and of blood-relationship has already been referred to. Young hosts prove most favorable, as for all transplantable tissues, normal or neoplastic. Hosts which are ill of causes that involve emaciation are relatively unfavorable, a circumstance noted in its relation to mammalian growths by other workers, and especially interesting because these hosts are more susceptible, as a rule, to the frankly infectious processes. A certain proportion of hosts, although of the proper variety for the tumor's growth, manifest a resistance such that it does not develop when implanted in them; while others in which the growth has developed and retrogressed are completely resistant, for a time at least. Similar types of resistance have already been demonstrated for the tumors of rats and mice. Furthermore they

have been elicited against a normal tissue capable of growth on transplantation (embryonic tissue).

Taken together the foregoing traits identify the chicken sarcoma as a typical malignant tumor. In them there is nothing to suggest the presence of a parasitic cause for the disease, but much that has been held to favor the view of an intrinsic cell-derangement.

For the first attempts to separate out the sarcoma's cause filtration was employed. The tissue of a rapidly growing tumor was ground with sand, taken up in Ringer's solution, shaken for some time, centrifugalized, and the supernatant fluid was passed through a Berkefeld filter which held back small bacteria. The injection of a few cubic centimeters of the limpid fluid thus obtained gave rise to the characteristic spindle-celled growth in fowls previously normal; and this growth was capable of further transplantation through an indefinite series of hosts. More recently the causative agent has been differentiated from the living tumor cells by drying, by glycerinization and by repeated freezing and thawing,—processes which the tumor cells fail to survive. It remains active for many months in dried sarcomatous tissue, and for at least one month in tissue placed in 50 per cent. glycerin. It is quickly rendered innocuous by temperatures above 53° C., by 50 per cent. alcohol, by 2 per cent. phenol, by saponin in high dilutions, by chloroform and toluol in the proportions which prevent bacterial growth during autolysis, and by autolysis itself. It will not pass through a dialyzing membrane, nor, in our experience, through a porcelain filter. These various features seem sufficient to identify it as a living organism in distinction from a ferment. The organism has never been directly observed in fresh or stained preparations; and the morphology of the individual tumor cells does not suggest its presence. Attempts to cultivate it *in vitro* have not as yet proven successful.

The neoplastic change brought about by the agent takes place slowly compared with the proliferation of the cells, once they have become sarcomatous. Growth of the tumor, dissemination, injury to the host, immune processes, all are referable to these cells suddenly endowed with new properties. The introduction into a susceptible fowl of a large amount of the filterable agent is not in itself

sufficient to cause a tumor. The development of a growth under these circumstances is conditional upon the presence of a cell-derangement, such, for example, as is produced by the injection of infusorial earth. Yet even when the element of cell-derangement has been supplied, and the agent injected in quantity, a considerable percentage of the fowls fail to develop a sarcoma. The nature of the factors responsible for this failure has not been determined. The importance of cell-derangement as a contributory cause of human sarcomata has long been recognized.

The chicken sarcoma is strikingly non-infective under ordinary conditions. During the last three years more than a thousand fowls, with or without the tumor, have been kept together in close quarters, yet no instance of natural transmission has been observed. An examination of numerous spontaneous chicken tumors from various sources has shown that the sarcoma is not epidemic. These facts find an explanation in the various factors by which the agent's action is conditioned.

In conclusion it should be stated that the experiments with the chicken sarcoma have not yielded a method whereby a causative agent can be separated from the tumors of rats and mice. But they clearly prove that the characteristics of malignant tumors in general are compatible with the presence of a living causative agent. Such a cause for them seems, indeed, far from improbable.

Note: Dr. James B. Murphy has shared, as joint author, in the work on the chicken sarcoma since the recognition of the latter's filterable cause; and more recently Dr. W. H. Tytler has aided in the study of some of the growth's problems.

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NEW YORK.

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THE PROTEIN POISON.

By VICTOR C. VAUGHAN, M.D.

(*Read April 19, 1912.*)

For many years I have been studying the chemistry of the bacterial cell. In 1900 I devised the large tanks for growing massive cultures. These have proved quite satisfactory, and I have been able to get bacterial cellular substances quite free from all impurities, in large amount. After many years of unsuccessful effort Wheeler and I, in 1903, succeeded in partially isolating the poisonous group from the cellular substance of certain pathogenic bacteria. This we did by heating the cellular substance with a two per cent. solution of sodium hydroxid in absolute alcohol. When this is done at the temperature of boiling alcohol the cell substance is split up into a poisonous and a non-poisonous part. The former is soluble in alcohol, while the latter is insoluble. This gives us not only a method of preparation, but also one of partial separation. I may say that the evidence that a distinct cleavage of the bacterial cell is secured is shown by the fact that all the carbohydrate and all the phosphorus in this cellular substance remains in the insoulble or non-poisonous part. The poisonous portion contains no phosphorus and no carbohydrate, but it does give the biuret and the Millon reaction, and must therefore be classed as a protein. This protein has never been obtained as yet in a state of chemical purity. The best preparation that we have been able to secure up to this time kills guinea pigs when injected intravenously, in doses of .5 of a mg. There are certain reasons for believing that its effect upon man is still more pronounced.

Having found this poison in pathogenic bacteria we next looked for it in non-pathogenic organisms, and we found it in these quite as abundantly as in the pathogenic forms. It therefore follows that the pathogenicity of the bacterial cell does not depend upon

its capability of producing a poison, because all bacterial cells contain a poison. Whether a germ is pathogenic to a given species of animal or not depends upon its capability of growing and multiplying in that animal's body.

Next we looked for this poison in certain animal proteins, such as the white of egg, the proteins of blood serum, the casein of milk, etc. In all of these the same or a like poison was found by the same method. Later we tested vegetable proteins, such as the gluten of flour, the zein of corn meal, the edestin of hemp seed, etc. Up to the present time we have examined more than thirty proteins of bacterial, animal and vegetable origin, and in all of these the same poisonous group has been detected.

It has long been suspected, and indeed I may say, known, that the protein molecule contains a poisonous group. At first it was supposed that the diverse proteins which man takes in his food are but slightly altered in the alimentary canal, and before absorption. It is now known that this is not true, and that in the healthy man all proteins are broken down into amino acids by the ferments of the alimentary canal, and that these amino acids are, either during absorption or directly thereafter, resynthesized so as to form the proteins which are characteristic of man's body. The precipitin test has demonstrated that every species of animal has its own specific protein bodies. Every albuminous molecule contains a poisonous group. Peptones injected into the blood act as poisons; therefore the peptone group contains a poisonous molecule, and it is this poisonous molecule in the peptone group which we have succeeded in partially isolating. The symptoms induced by this protein poison are marked and characteristic. They divide themselves into three distinct groups. Soon after the injection of a minimum fatal dose in one of the lower animals there is evidence of peripheral irritation. This is shown by the fact that the animal becomes restless and attempts to scratch itself, not only the part adjacent to the point of injection, but every portion of its body which it can reach. This is known as the stage of peripheral irritation. In man it is characterized by itching and by an erythematous eruption which begins about the place of injection, and rapidly spreads over the

body. In the second stage the animal lies in a lethargic condition, with rapid, difficult respiration. It prefers not to move, and when urged to do so it shows that it has partially lost the power of coördinating its movements. It drags its posterior extremities, or it sways from side to side. This is known as the paralytic stage. The third stage manifests itself by clonic covulsions, which repeat themselves after intervals of rest, becoming more and more violent, until death results. After reaching the convulsive stage recovery is rare, although it does occasionally occur. The symptoms are produced by the injection of protein poison, whether obtained from bacterial, animal or vegetable proteins. It should be stated that in order to study these symptoms properly the dose should approach the minimum quantity. When the dose is excessive the first and even the second stage may not be observed. The animal is speedily thrown into a convulsion, and death results within a few minutes. When a non-fatal dose is given the first and second stages appear, and may last in guinea pigs for an hour, possibly two, but recovery is rapid and apparently complete. It is of importance to note this fact, that when recovery does take place it follows rapidly, and apparently the animal is as well as ever within two or three hours, and possibly earlier.

We had studied this protein poison and its effects upon animals when the phenomenon of protein sensitization, improperly called anaphylaxis, was discovered. All will understand that protein sensitization is demonstrated by injecting a protein, any protein, into an animal and waiting for a certain length of time, or until the animal becomes sensitized, when a second injection into the same animal causes the symptoms which I have described, in the same order as observed when the protein poison is administered, and that the final effects are the same. Comparing the phenomena of protein sensitization with those of protein poisoning Wheeler and I in 1907 offered the following explanation of protein sensitization: When a foreign protein is injected into an animal it must be disposed of in some way. Unless introduced in large amount it is not eliminated by the kidney. It soon disappears from the circulating blood and is deposited in various tissues, the exact place of deposition depending

upon the kind of protein injected and the species of animal. In order to deal with this foreign material certain body cells develop a specific proteolytic ferment, which splits up the protein injected, and no other. The first dose is gradually split up, and consequently produces no recognizable effect upon the animal. When a proper interval of time is allowed to elapse before the second injection, this new ferment, in the form of a zymogen is stored up in certain cells of the body, and when the second injection of the same protein is made this zymogen is activated, and converted into a ferment which splits up the injected protein with great rapidity, setting free the same poison which we obtained by splitting up proteins with sodium hydroxid in absolute alcohol. This explanation of the phenomena of protein sensitization was published by Wheeler and myself in 1907. Recently it has been confirmed in France by Nicolle and Abt, and in Germany by Friedberger. It is true that Friedberger does not fully give us credit for this work. He says that we suggested this explanation, and he has demonstrated it. It is unfortunately true that much of the scientific work done in America must go to Germany and be approved before it is accepted by other Americans. This is due to our lack of confidence in ourselves and in one another. The German has so long been accustomed to stamp his products as "made in Germany," that much of our scientific work comes back with this stamp upon it. However, it is not my purpose to complain about this matter. My European confreres have given me, on the whole, fair credit for work done along this line.

More recently we have attempted to use the knowledge which we have gained in the study of the protein poison in the explanation of many of the phenomena of immunity and of disease. The essential difference between egg-white and the typhoid bacillus is that the former is a non-labile, dead protein, while the bacillus is a labile, living protein. If egg-white could grow and multiply after being introduced under the skin, or into the blood of an animal, it would be just as dangerous to prick a finger with a needle moistened with this relatively harmless, bland protein as it would to inoculate oneself with the anthrax bacillus. As early as 1907 Wheeler and I held

that protein sensitization and bacterial immunity are one and the same thing. In sensitization the animal dies on the second dose. In immunity the animal survives the second dose. Sensitization and immunity are therefore apparently antipodal, but are in fact the same thing. A man drinks water containing the typhoid bacillus, and he does not develop typhoid fever that day, nor the next. He passes through a period of incubation, which in typhoid fever is somewhere about eight or ten days. During this time the typhoid bacillus is multiplying in his body in great numbers, and in doing so it is converting his proteins into typhoid proteins. Suddenly the period of incubation stops and the disease begins to manifest itself. The period of incubation stops when the body cells have become sensitized to the typhoid protein, and begin to break it up. From that time on the fight is between the living cells of the body with the ferment which they pour out, and the bacilli.

It occurred to us that if this theory be true we might demonstrate it by repeated injections of small quantities of some protein body, and determine what effect such injections might have upon body temperature. In these experiments we have used egg-white principally because we wanted to get away from cellular structure and from the supposed influence of life. We wanted to take a dead substance. Of course in doing so we recognized the fact that egg-white does not grow and multiply in the body, and consequently we must keep up the supply by repeated injections. I have published several papers upon this, notably in the *Zeitschrift für Immunitätsforschung,* and therefore I am relieved from the necessity of going into detail in this article. Suffice it to say that by varying the size of the dose and the interval between the doses one can induce in the lower animals any kind of fever that one wishes. One can place an animal in a typhoid condition, and by repeated injections keep the animal in this condition with a temperature identical with that of typhoid fever for days and weeks. On the other hand, by more frequent injections one can induce in a rabbit an acute, fatal fever, terminating in a few hours; or, by again varying the size of the dose and the interval, one can secure at will the picture of remittent or intermittent fever. Fever, therefore, results from the introduc-

tion of a foreign protein into the body, the sensitization of the body cells to that protein, and finally the cleavage of that protein by the ferment elaborated by the sensitized body cells. Now in nature practically all the proteins that find their way into the body undigested are living proteins, in the form of bacteria or protozoa. They grow and multiply in the body, without materially disturbing for the time being, the life of the individual. This continues during the period of incubation but when the body cells have become sensitized and begin to split up the foreign protein the period of incubation ceases and that of disease begins.

We have shown that repeated injections of foreign protein not only cause fevers of various kinds, but lead to emaciation of the animal body, to increased elimination of nitrogen, and to decreased urinary secretion, and, in short, to all the phenomena that are characteristic of the febrile diseases. Death from any of the infectious diseases is due to one and the same poison, and that poison is a constituent of the protein molecule. Symptoms vary in different diseases for two reasons: In the first place, the foreign proteins have different predilection places in the body in which they are deposited. In the second place the ferment which splits up these foreign proteins is specific for different diseases. The most successful diagnostician cannot determine the nature of the bacterial organism which causes the symptoms of meningitis. The symptoms are the same so long as the organ involved is the same. The meningitis may be due to the meningococcus, to the streptococcus, to the typhoid bacillus, or to the tubercle bacillus. Still, the symptoms are the same because the cleavage of the foreign molecule occurs in the same part of the body. Again, every medical man knows how difficult it is to distinguish between typhoid fever and acute general miliary tuberculosis, because in both instances the foreign protein is largely in the blood current. As I have stated, most bacterial proteins have predilection places in which they are deposited. The typhoid bacillus prefers the mesenteric and other glands; the pneumococcus is deposited generally in the lungs, though it may be found in the intestinal walls. The meningococcus finds its favorite place for growth and development in the coverings of the brain. The tubercle bacil-

lus grows most frequently in the lungs, though it has fed upon man for so long a time that it is now able to sustain itself in almost any part of his body.

From what has been said it must follow that fever on the whole is a beneficent process. It is one of the phenomena of the parenteral digestion of proteins. The foreign protein has gotten into the body, is growing and multiplying, and in doing so is utilizing the proteins of man's body. It must be destroyed, and the body cells pour out a ferment which digests the foreign protein. This is nature's way of disposing of the foreign material, and it is apparently about the only way that nature has of doing it. I repeat therefore that fever on the whole is a beneficent process. It is an attempt on the part of nature to get rid of the invading protein. Like many other of nature's processes it may be overdone, and death may result from fever, per se.

That fever does result from a fermentative cleavage is shown not only by the facts which I have already enumerated, but those which we have learned in combating fever. Nearly all, if not all, of the anti-febrile reagents which have been employed in medicine are anti-ferments, and they lower the temperature by retarding the process of protein cleavage. Both natural and acquired immunity, apart from toxic immunity, may be explained by the facts as stated above. In natural immunity the foreign protein is either unable to grow and multiply, and this means that its ferments are unable to split up the proteins of the body, or the ferments of the body split up the invading protein before it has time to grow and multiply. This explains natural immunity, whether it be racial or individual.

Acquired immunity is explained by the fact that the first attack of the disease, or inoculation with a modified virus, develops in the body cells a ferment which is stored up, and which on a second injection of the same protein, acts rapidly, and effectively, and splits up the invading virus. In vaccination for smallpox we use a virus modified by its passage through the cow. This modified smallpox virus develops in the body cells a ferment which is capable of splitting up the smallpox virus, and the next time this individual comes in contact with a smallpox patient, or receives the smallpox virus, it is split up and destroyed before it has time to grow and multiply.

This also explains the beneficial effects that undoubtedly have been obtained by the various vaccines now so widely and often so unintelligently used.

I wish to suggest that the exanthematous diseases may be explained by the fact that the foreign proteins of certain diseases are deposited in the skin, and that this tissue is the site of the destruction of the foreign body. I may say in support of this that we have repeatedly injected egg-white into the ear vein of rabbits. After varying periods of time we have shown by sensitizing animals with blood taken from the heart that the egg-white has wholly disappeared from the circulating blood of the rabbit. Later it can be shown that this egg-white has been deposited in the skin, in the kidney, in the brain, and in various other organs of the rabbit. It seems to me that our work upon the protein poison furnishes us with facts, by means of which we are able to explain many of the phenomena of immunity and of disease.

SOME GEOCHEMICAL STATISTICS.[1]

By FRANK WIGGLESWORTH CLARKE.

(*Read April 20, 1912.*)

More than twenty years ago, in a paper on the relative abundance of the chemical elements,[2] the present writer compared a number of averages of analyses of igneous rocks, representing different regions, and showed that they were essentially identical. From these averages, combined into a general average, the mean composition of the igneous part of the lithosphere was computed, and the result obtained has since been confirmed by the study of much larger masses of data than were originally attainable.[3] Other estimates, made by other computers upon similar lines, have since served to check my own, thereby giving to my conclusions a high degree of probability. The figures obtained have received a fairly general acceptance, and have served as a basis for other computations of a fundamental character.

This acceptance, however, has not been universal. The process of averaging analyses is criticized by several writers,[4] who urge that it is unphilosophical. An analysis of a dike rock is given the same weight as that of a widespread and important formation, whereas each rock should be weighted in accordance with its volume. But we do not and probably cannot know these volumes, partly because detailed surveys are lacking, and partly because a surface outcrop fails to tell us what bulk of rock may lie below. If we try to estimate the volumes of the many rocks represented in the average, or

[1] Published by permission of the Director of the U. S. Geological Survey.

[2] *Bull. Phil. Soc. Washington,* 1889, Vol. 11, p. 131. Also in Bull. 78, U. S. Geological Survey, 1891, p. 34.

[3] See Bull. 491, U. S. Geological Survey, "The Data of Geochemistry," pp. 22–27.

[4] See, for example, Daly, *Proc. Amer. Acad.,* 1910, Vol. 45, p. 211; Loewinson-Lessing, *Geol. Mag.,* 1911, p. 248; and Mennell, *Geol. Mag.,* 1904, p. 263, and 1909, p. 212.

even the areas exposed, we shall find ourselves relying in great part upon arbitrary assumptions, a procedure fully as unphilosophical as that which it would supplant. Estimates of that and similar kinds have been made, most recently by Loewinson-Lessing, whose figures give essentially the same result as that obtained by the method he has criticized. The method by volumes is doubtless ideal, but impracticable; and the true, philosophical procedure is to do the best we can with the available data. It is highly probable that the rocks of minor importance will balance one another, the persilicic and subsilicic varieties occurring in something like equal proportions. This supposition is sustained by the groups of average analyses which will presently be given. If we trust to individual judgments, different observers will reach widely different conclusions. Loewinson-Lessing supposes that the average rock may be about the mean of an average granite and an average basalt; Daly[5] argues in favor of a fundamental basaltic magma; Mennell, whose experience has been gained in a granitic region, regards granite as the dominant rock with all else of minor importance. Mennell makes a strong argument in favor of his contention; but there is weighty evidence against it. If we study recent lavas, that is, the rocks which issue from unknown depths far below the surface, we shall see that rhyolite, the effusive equivalent of granite, is much rarer than andesite or basalt. The Deccan trap, the Columbia River basalt, the andesites of South America, the lavas of Iceland and the Hawaiian Islands are good illustrations of this statement. Moreover, the river waters which originate in areas of crystalline rocks contain almost invariably an excess of lime over soda, which would hardly be the case were granite predominant. Much so-called granite is really either quartz diorite or quartz monzonite, rocks which are probably far more abundant than has been commonly supposed.

In order to test the method of averaging analyses we may now compare the averages so far obtained by different computers, and then pass on to averages of rocks from distinct and widely separated areas. In these averages only the more important constitu-

[5] Bull. U. S. Geol. Survey, No. 209, 1903, p. 110.

ents of the rocks are considered, for the reason that the less conspicuous components have not been generally determined. They will be separately discussed later. All the means have been recalculated to 100 per cent., and water, for obvious reasons, is excluded. The methods for the determination of water are far from being uniform, and the variations are so great as to obscure the essential agreements between the other figures. The first table contains the following averages.

(*A*) The average of 248 "superior" analyses of igneous rocks selected by Washington from Roth's tables. See U. S. Geol. Survey, Prof. Paper No. 28. Average computed by the present writer.

(*B*) The average of 536 British rocks, computed by Harker. "Tertiary Igneous Rocks of the Isle of Skye," Mem. Geol. Survey United Kingdom, 1904, p. 416.

(*C*) Washington's average of 1,811 rocks from all parts of the world. U. S. Geol. Survey, Prof. Paper No. 14, p. 106.

(*D*) Loewinson-Lessing's estimate of a mean between an average granite and an average basalt. Inserted here for comparison with the other columns.

(*E*) The average of all the data relating to the composition of igneous rocks contained in the laboratory records of the U. S. Geological Survey. Computation by F. W. C.

	A	*B*	*C*	*D*	*E*
SiO_2	61.13	60.49	58.83	60.17	61.68
Al_2O_3	15.03	15.81	15.97	15.43	15.41
Fe_2O_3	3.50	4.90	3.36	3.55	2.67
FeO	4.38	2.77	3.91	4.07	3.55
MgO	2.86	3.81	3.88	3.62	3.97
CaO	4.81	4.95	5.27	5.63	5.02
Na_2O	3.75	3.26	3.95	3.24	3.49
K_2O	3.32	2.84	3.19	2.82	3.07
TiO_2	.78	.53	1.05	.89	.77
P_2O_5	.25	.22	.37	.35	.27
MnO	.19	.42	.22	.23	.10
	100.00	100.00	100.00	100.00	100.00

The general agreement is striking, and Loewinson-Lessing's estimate fits well in with the others. The next table is devoted to the

igneous rocks of North America, and the analyses are nearly all taken from the Survey records.

(*F*) Average of 250 analyses of rocks from the Atlantic slope, Maine to Georgia. 78 of these are taken from Washington's tables, the others were made in the Geological Survey.

(*G*) Average of 113 analyses of rocks from the Yellowstone Park.

(*H*) Average of 137 analyses of rocks from Colorado.

(*I*) Average of 195 analyses of rocks from California.

(*J*) Average for all North America. The figures of column *E* combined with those of 398 analyses given in Washington's tables.

	F	*G*	*H*	*I*	*J*
SiO_2	59.16	61.22	62.87	62.20	61.47
Al_2O_3	15.17	16.35	16.66	15.87	15.63
Fe_2O_3	2.16	3.30	2.67	2.12	2.67
FeO	5.36	2.58	2.40	3.46	3.22
MgO	4.92	3.47	2.06	3.93	4.02
CaO	5.69	4.95	4.01	6.06	5.12
Na_2O	3.46	3.83	4.11	3.49	3.62
K_2O	2.76	3.22	4.10	1.92	3.06
TiO_2	.98	.69	.74	.58	.81
P_2O_5	.23	.31	.27	.27	.27
MnO	.11	.08	.11	.10	.11
	100.00	100.00	100.00	100.00	100.00

For Europe the following data are sufficient, all the figures, except Harker's, having been taken from Washington's tables.

(*K*) Harker's average for British rocks, as previously cited.

(*L*) Average of 231 analyses of rocks from Norway, Sweden and Finland.

(*M*) Average of 420 analyses of rocks from the German and Austro-Hungarian Empires.

(*N*) Average of 250 analyses of Italian rocks.

(*O*) General mean of the foregoing 1,427 analyses, plus 122 of rocks from parts of Europe not otherwise covered.

Good analyses of rocks from Asia, Africa and Australia, at least as given by Washington, are too few for satisfactory combination. In the next table I give a general average for North

	K	*L*	*M*	*N*	*O*
SiO_2	60.49	60.84	58.83	60.18	59.89
Al_2O_3	15.81	16.70	15.76	15.88	16.07
Fe_2O_3	4.90	3.75	3.96	3.90	4.18
FeO	2.77	2.68	3.75	3.44	3.18
MgO	3.81	2.10	4.09	2.82	3.59
CaO	4.95	4.39	5.73	5.83	5.30
Na_2O	3.26	4.98	3.74	3.36	3.70
K_2O	2.84	3.68	3.18	4.17	3.25
TiO_2	.53	.63	.67	.10	.51
P_2O_5	.22	.14	.22	.21	.20
MnO	.42	.11	.07	.11	.13
	100.00	100.00	100.00	100.00	100.00

America, South America and Europe, South America being represented by the average of 82 analyses cited by Washington. The weights assigned are stated under each column.

	J, N. America.	*P*, S. America.	*O*, Europe.	*Q*, General Mean.
SiO_2	61.47	61.13	59.89	60.76
Al_2O_3	15.63	16.29	16.07	15.87
Fe_2O_3	2.67	3.76	4.18	3.40
FeO	3.22	2.93	3.18	3.19
MgO	4.02	3.18	3.59	3.78
CaO	5.12	5.54	5.30	5.23
Na_2O	3.62	4.01	3.70	3.67
K_2O	3.06	2.16	3.25	3.08
TiO_2	.81	.66	.51	.67
P_2O_5	.27	.14	.20	.23
MnO	.11	.20	.13	.12
	100.00	100.00	100.00	100.00
Weight	8	1	7	

The last column may be taken as probably representing, with certain obvious limitations, the average igneous rock of the entire visible position of the lithosphere. The agreement between the preceding columns is so close as to suggest that similar averages from other parts of the world are likely to be of the same general order. It is hardly conceivable that analysts, dealing with rocks from such diverse regions as Colorado, South America, Germany, Great Britain, etc., should *select* subsilicic and persilicic rocks or salic and ferric rocks, in nearly identical proportions. The selection has been made by nature itself, and although there are differences indicating the existence of petrographic provinces, they are so slight as to leave

the essential identities unobscured. Some human selections are indicated in certain regions; for example, in Norway, the high soda is due to the disproportionate attention paid to the nepheline syenites; while in Italy the leucite rocks of Vesuvius lead to an apparent excess of potash. In the mean for all Europe these differences balance each other.

The general agreement between the several averages is well brought out by means of the method developed in the quantitative classification of igneous rocks. For this comparison, Professor Iddings has kindly made the necessary computations, with the subjoined results: Duplications of averages are of course avoided.

Average.	Column.	Magmatic Symbol.	Name.
Roth	*A*	II. 4. 2. 3.	Adamellose.
Harker	*B, K*	II. 4. 3. 3.	Harzose.
Washington	*C*	II. 5. 2. 3.	Monzonose.
Loewinson-Lessing	*D*	II. 4. 3. 3.	Harzose.
Survey	*E*	II. 4. 3. 4.	Tonalose.
Atlantic Coast	*F*	II. 5. 3. 4.	Andose.
Yellowstone Park	*G*	II. 4. 3. 4.	Tonalose.
Colorado	*H*	II. 4. 2. 3.	Adamellose.
California	*I*	II. 4. 3. 4.	Tonalose.
North Europe	*L*	II. 5. 2. 4.	Akerose.
Central Europe	*M*	II. 5. 3. 4.	Andose.
Italy	*N*	II. 5. 2. 3.	Monzonose.
N. America	*J*	II. 4. 3. 4.	Tonalose.
S. America	*P*	II. 5. 3. 4.	Andose.
All Europe	*O*	II. 4. 3. 4.	Tonalose
General mean	*Q*	II. 4. 3. 4.	Tonalose.

In brief, all the averages fall in class II., dosalane, and in orders 4 and 5, but near the boundary between the two. The principal variations are in the rangs and sub-rangs, and are mainly due to the varying proportions of the alkalies. The average rock may be classed as tonalose, or, to use a more familiar term, as andesite. It is evident that in order to have any statistical validity, such averages as are given here must represent fairly large areas, and a considerable number of analyses. Small areas, especially those of volcanic islands, may vary widely from the mean; for example, analyses of 32 Hawaiian rocks gave in the average, only 48.55 per cent. of silica. Such local variations, however, could hardly exert any noteworthy influence upon the continental averages which represent

more truly the general magma. They would be balanced, or more than balanced, by other areas of granitoid or dioritic character.

The tables so far given serve only to show the probable uniformity of the accessible magmas. In order to complete them, a number of minor constitutents of igneous rocks must be considered; which, however, have only been determined with adequate frequency in the laboratory of the United States Geological Survey. For example, in round numbers, 1,100 nominally complete analyses have here been made, and in 736 of them barium oxide was determined or proved to be absent. The mean of these determinations, counting absences as zero, is 0.100 per cent., which may be a maximum. If the remaining 364 rocks were all free from barium, and so regarded, the mean percentage of BaO would be 0.067, a minimum. Between these two figures the most probable value would lie, but nearer the maximum than the minimum, the mean being 0.084. Upon this basis of computation the following table of percentages has been constructed.

	No. of Determ.	Maximum.	Minimum.	Mean.
BaO	736	0.100	0.067	0.084
SrO	616	0.040	0.022	0.031
Li_2O	577	0.011	0.005	0.008
NiO	287	0.027	0.007	0.017
Cr_2O_3	270	0.050	0.012	0.031
V_2O_3	87	0.024	0.002	0.013
S	748	0.112	0.076	0.094
F	98	0.100	0.010	0.055
Cl	260	0.062	0.015	0.038
ZrO_2	305	0.025	0.007	0.016
CO_2	662	0.490	0.330	0.410

It is evident that these figures are not of equal significance. Some of them rest upon too few determinations, especially those for fluorine and vanadium. The experience of the Survey laboratory, however, leads me to believe that vanadium is very widely diffused in the igneous rocks, and that the mean assigned to it may possibly be exceeded. Chromium, nickel and zirconium are also more abundant than they were formerly thought to be. As for carbon dioxide, the figure given is probably much too high, for its presence in igneous rocks is mainly but not entirely ascribable to

alteration. The maximum for CO_2, 0.49 per cent., may nevertheless be admitted in computing the composition of the entire lithosphere, as an allowance for carbonaceous matter which would otherwise escape attention. Copper does not appear in the table, but it is sometimes determined, and the order of its abundance is perhaps about the same as that of lithium, say 0.01 per cent. Boron, possibly, ought also to be taken into account, but it is seldom estimated, and no figure can be assigned to it. As for water, which up to this point has been neglected, the average shown by the Survey analyses is 1.95 per cent. This figure, which is excessive so far as the unaltered igneous rocks are concerned, may, however, be taken as including the water enclosed in the lithosphere and not otherwise estimated. The allowance is ample.

Including the minor constitutents, the Survey analyses alone give the subjoined mean composition for that portion of the lithosphere which is represented by rocks of igneous origin. The total is recalculated to 100 per cent.

SiO_2	59.85
Al_2O_3	14.95
Fe_2O_3	2.59
FeO	3.45
MgO	3.85
CaO	4.87
Na_2O	3.39
K_2O	2.99
H_2O	1.92
TiO_2	.75
CO_2	.48
P_2O_5	.27
MnO	.10
Minor constituents	.54
	100.00

This differs from the general mean already given under column *Q* in that it includes only the Survey data, and takes account also of water, carbon dioxide, and the minor constituents of igneous rocks. The important values, however, are all of the same order of magnitude in both estimates. The undetermined elements cannot aggregate over 0.5 per cent., and represent minor corrections which may

be considered at some future time when suitable data are at hand. For present purposes they are negligible.

The mean composition of the sedimentary rocks has been determined in a different way. Composites of many samples were prepared and analyzed as if they were single samples, a method which saved much labor and was as satisfactory as if numerous individual analyses had been made. The data thus obtained have been repeatedly published, except in the case of the shales, for which a new estimate is here given. The analyses, recalculated to 100 per cent., are as follows:

(*A*) Composite of 78 shales plus 45 individual analyses taken from the laboratory records of the U. S. Geological Survey. 123 shales in all.

(*B*) Composite analysis of 253 sandstones.

(*C*) Composite analysis of 345 limestones.

	A	*B*	*C*
SiO_2	59.23	78.33	5.18
Al_2O_3	15.68	4.76	.81
Fe_2O_3	3.59	1.07	} .54
FeO	2.85	.30	
MgO	2.71	1.16	7.90
CaO	2.58	5.49	42.57
Na_2O	1.27	.45	.05
K_2O	3.14	1.31	.33
H_2O	4.97	1.66	.77
TiO_2	.65	.25	.06
CO_2	2.40	5.02	41.54
P_2O_5	.15	.08	.04
MnO	.05	——	.05
Minor constituents	.73	.12	.16
	100.00	100.00	100.00

These three columns represent the three dominant types of sedimentary rocks, and differ from the figures given the igneous rocks in certain losses by leaching, mainly of lime, magnesia and alkalies, and in a gain of water and carbon dioxide received from percolating waters and the atmosphere. There is a small increase in volume consequent upon the gains; the materials lost are principally to be sought for in that universal reservoir, the ocean.

For the relative proportions of the sedimentary rocks several

estimates have been made, which, however, can only be regarded as rough approximations to the truth. By studying the way in which an average igneous rock can break down, I have computed that the shales form 80 per cent., the sandstones 15 per cent., and the limestones 5 per cent. of the total.[6] Van Hise[7] distributes them as 65, 30 and 5 per cent. respectively, and Mead,[8] by a graphic process, has found 80, 11 and 9 per cent. Combining the composite analyses just given in accordance with these ratios the following mean values are obtained for the total composition of the sedimentaries. The fourth column contains the average of the three separate estimates.

	Clarke.	Van Hise.	Mead.	Average.
SiO_2	59.39	62.29	56.47	59.38
Al_2O_3	13.30	11.67	13.14	12.70
Fe_2O_3	3.06	2.69	3.04	2.93
FeO	2.33	1.94	2.31	2.19
MgO	2.74	2.50	3.00	2.75
CaO	5.01	5.46	6.50	5.65
Na_2O	1.09	.96	1.07	1.04
K_2O	2.73	2.45	2.69	2.63
H_2O	4.27	3.77	4.23	4.09
TiO_2	.56	.42	.55	.51
CO_2	4.75	5.15	6.21	5.37
P_2O_5	.13	.12	.13	.13
MnO	.05	.05	.05	.05
Minor constituents	.59	.53	.61	.58
	100.00	100.00	100.00	100.00

The first of these averages is the most probable, since it harmonizes best with the mean composition of the igneous rocks. If we reduce it to the form adopted for the latter, by rejecting its accessions, carbon dioxide and water, and also neglecting the minor constituents, it may be compared with the average marked *E* as follows:

	Igneous *E*.	Sedimentaries.
SiO_2	61.68	65.70
Al_2O_3	15.41	14.72
Fe_2O_3	2.67	3.38
FeO	3.55	2.58
MgO	3.97	3.03
CaO	5.02	5.54

[6] U. S. Geol. Surv., Bull. 491, p. 31.
[7] U. S. Geol. Surv., Monograph 47, p. 940.
[8] *Journ. Geol.*, 15, 238.

	Igneous *E*.	Sedimentaries.
Na_2O	3.49	1.21
K_2O	3.07	3.03
TiO_2	.77	.62
P_2O_5	.27	.14
MnO	.10	.05
	100.00	100.00

This comparison shows an increase in silica, due to concentration. Alumina is lowered, iron is nearly constant although the proportions of the two oxides are reversed, and potassium is constant also. Soda and magnesia show losses due to leaching. Lime is increased, which suggests that the 5 per cent. of the sedimentaries assigned to limestone is probably too high. Mead's estimate of 9 per cent. is certainly excessive. The largest uncertainty is due to the facts that the composite shale used for analysis did not include certain magnesian rocks, and the limestones did not embrace the dolomites. The deficiency of magnesia, however, is mainly to be found, with the soda, in the ocean. On the whole the comparison is satisfactory, although the lowering of the alumina is unexplained. The least satisfactory feature of the combination is the average composition of the shales; but even here the percentage of soda is probably good. In the important igneous rocks the proportion of soda varies within narrow limits, and any combination of them would yield sedimentary residues in which that oxide should show much the same relative loss. The Survey average for soda (column *E*) gives 3.49 per cent. The mean of 100 rhyolites given in Washington's tables yields 3.48 per cent., while for 220 basalts the percentage is 3.24. From the preceding table the ratio between igneous and sedimentary soda is 1.21 to 3.49, an order of magnitude which is not likely to be very much changed. The influence of certain variations in it will be considered later.

From the quantity of sodium in the ocean the mass and volume of igneous rock needed to furnish it are easily computed, and also the similar data for the sedimentaries. The oceanographic data are fortunately quite good, even though they may not be rigorously exact. The mean density of sea water, according to Murray, is 1.026, and its average proportion of saline matter is 3.5 per cent. by

weight. For the volume of the ocean there are several estimates. In my former calculations it was taken as 302,000,000 of cubic miles, while Murray found a larger figure, 323,722,150 cubic miles. These values, however, are now supplanted by later and more definite estimates, as follows: According to Karstens,[9] the volume of the ocean is 1,285,935,211 cubic kilometers, or 308,509,000 cubic miles. Krümmel,[10] still later, estimates the volume as 1,329,945,870 cubic kilometers, or 319,087,500 cubic miles. From these figures, with density 1.026, the mass of the ocean is as follows:

Karstens, 1,319,369,526,436,000,000 metric tons.

Krümmel, 1,364,524,469,802,000,000 metric tons.

The saline matter, 3.5 per cent., is therefore:

Karstens, $46{,}178 \times 10^{12}$ metric tons.

Krümmel, $47{,}758 \times 10^{12}$ metric tons.

Puting the specific gravity of the oceanic salts at 2.25, their volume is easily found by the subjoined equation:

$$\frac{3.5\,V \times 1.026}{100 \times 2.25} = x,$$

in which V represents the volume of the ocean. The value of x then is,

With Karstens' volume 4,923,800 cubic miles.

With Krümmel's volume 5,092,600 cubic miles.

The second of these values, derived from Krümmel's estimate of the volume of the ocean, represents a quantity of saline matter which would cover the entire surface of the earth, 197,000,000 square miles, with a layer 136.5 feet deep, or 468 feet over the land area alone. Figures like these show that the salts enclosed in the rocky crust of the earth are, in amount, relatively insignificant.

From Dittmar's analyses of sea water, made for the reports of the Challenger Expedition, the mass of each radicle contained in the oceanic salts can be calculated. In the following table I give Dittmar's percentage composition of the salts, and in a second column the weights corresponding to the total mass of $47{,}758 \times 10^{12}$ metric tons as found from Krümmel's figure the volume of the ocean.

[9] Inaugural Dissertation, Kiel, 1894.

[10] Encyclopedia Britannica, 11th ed., Vol. 19, p. 974.

	Per Cent.	Metric Tons $\times 10^{12}$.
Cl	55.292	26,406
Br	.188	89.8
SO_4	7.692	3,673.5
CO_3	.207	98.8
Na	30.593	14,611
K	1.106	528.2
Ca	1.197	571.7
Mg	3.725	1,779
	100.00	47,758

The average specific gravity of 958 igneous rocks, collected in Washington's Tables, is 2.737; and one cubic kilometer of rock should therefore weigh 2,737,000,000 metric tons. From the average of the Survey analyses given on p. 221 *ante*, the mean percentage of sodium in igneous rocks is 2.51, and from Washington's average 2.90. Hence the sodium contained in the ocean, $14{,}611 \times 10^{12}$ metric tons, would be furnished by the complete decomposition of the following volumes of igneous rock:

From the Survey average, 212,680,000 cubic kilometers.

From Washington's average, 184,080,000 cubic kilometers.

Some sodium, however, remains in the sedimentary rocks, namely, 0.90 per cent. as computed from the *reduced* average analysis. On comparing this figure with those for the total sodium of the igneous rocks, the volumes of the latter actually decomposed become, approximately,

Survey, 318,950,000 cubic kilometers.

Washington, 260,160,000 cubic kilometers.

In order to determine the volume of the sedimentaries these figures require two small corrections. First, a deduction must be made for the soda, potash, lime and magnesia lost during erosion, and now represented by the corresponding radicles in the ocean. Secondly, for the difference in density between the original rocks and the sedimentaries, indicated by the ratios 2.737 : 2.6. The first correction is approximately $24{,}095 \times 10^{12}$ metric tons, or 8,803,000 cubic kilometers. Making the corrections, we have, for the actual volume of the sedimentaries,

Survey, 326,590,000 cubic kilometers, or 78,338,000 cubic miles.

Washington, 264,600,000 cubic kilometers, or 63,481,000 cubic miles.

The higher of these figures is equivalent to a shell of rock completely enveloping the earth, 2,100 feet thick, or covering the land area to the depth of 7,198 feet. This estimate is probably a maximum, but it gives fairly well the order of magnitude of the volume to be determined. An exact estimate is of course unattainable, but the total volume of the sedimentary rocks cannot much exceed 80,000,000 of cubic miles, or roughly, one fourth the volume of the ocean. If we apportion the volume actually found between the different classes of the sedimentaries their volumes in cubic miles become:

Shales	62,662,400
Sandstones	11,749,200
Limestones	3,916,400
	78,328,000

This leads to the surprising conclusion that the volume of the limestones is less than that of the oceanic salts, or at least is a quantity of the same order of magnitude. If the fundamental igneous rock was more largely basaltic than the average analyses show, the proportion of limestone would be increased, but probably not to any very great extent. Some allowance should be made for sodium salts enclosed within the rocks; but it is easy to show that such a correction must be small. If all the sandstones had an average porosity of 20 per cent., and if its pore space, once saturated with sea water, retained all of its sodium, the total amount retained would be 110×10^{12} metric tons. This amount, which is evidently excessive, is only 0.75 per cent. of the sodium in the ocean. Its inclusion in the foregoing computations would raise the volume of rock decomposed to 78,100,000 cubic miles, an increase smaller than the unavoidable uncertainties of the computation.

The foregoing estimate of the volume of the sediments obviously includes those which cover a great part of the ocean floor, as well as those which are now on land. Some of the latter, indeed, were once oceanic deposits, and are now, by erosion, being partially returned to the sea, either mechanically as salt and sand, or dissolved in the water of rivers. An exact knowledge of the chemical work of river water is therefore of great statistical importance from several points

of view. In a former memoir[11] I have given a careful estimate of the composition of river waters, and of their entire dissolved load. This estimate is reproduced in the following table: Column I. gives the average composition of the inorganic matter carried in solution by rivers. In column II. the annual contribution of each radicle to the ocean is stated in metric tons.

	I. Per Cent.	II Metric Tons per Annum.
CO_3	35.15	961,350,000
SO_4	12.14	332,030,000
Cl	5.68	155,350,000
NO_3	.90	24,614,000
Ca	20.39	557,670,000
Mg	3.41	93,264,000
Na	6.40	175,040,000
K	1.51	41,299,000
$(Al, Fe)_2O_3$	2.75	75,213,000
SiO_2	11.67	319,170,000
	100.00	2,735,000,000

The figures in the second column represent the quantity of inorganic matter annually removed from 40,000,000 square miles of the earth's surface, the remaining area being, so far as additions to the ocean are concerned, practically negligible. Interior deserts, closed basins, and the circumpolar lands are left out of account. From 40,000,000 square miles of land, 2,735,000,000 tons of dissolved matter are carried each year to the sea. This quantity, however, is not a true measure of chemical denudation. The NO_3 is of atmospheric origin or else derived from organic sources, and 8 per cent. of the CO_3 represents recent accessions from the atmosphere. Much of the latter radicle is accounted for by the solution of limestones, and was once, of course, atmospheric, but it is now a part of the present erosion. Making the indicated deductions the total quantity of matter transported in solution is reduced to 2,495,585,000 tons annually; or 25×10^8 in round numbers. This, with a probable specific gravity of 2.6 has a volume of 0.96154 cubic kilometer, equivalent to a layer 0.000009281 meter deep, or 0.0003655 inch over the 40,000,000 square miles of land. At this rate the mean

[11] A preliminary study of chemical denudation. Smithsonian Misc. Coll., Vol. 56, No. 5, 1910.

surface of the continents would be lowered by solvent erosion alone, to the extent of one foot in 32,833 years. In some areas the rate is much more rapid, in others it is slower; but the average is as close as can be computed with the data now in hand. Its uncertainty may be as great as ten per cent., or perhaps even greater. The chief uncertainty is due to our lack of precise knowledge concerning the greater African and Asiatic rivers.

From the ratio between fluviatile and marine sodium the age of the ocean can be calculated. The ocean contains $14{,}611 \times 10^{12}$ metric tons of sodium, and the rivers contribute 175,040,000 tons annually. Hence, if the ocean were originally fresh, its entire content of sodium would be supplied by the rivers in 83,472,000 years. This form of calculation was first applied by Joly,[12] whose work is well known; and has since been discussed by Sollas[13] and also by myself in the memoir already cited. The quotient thus obtained, however, is subject to various corrections, which have been considered by the authors named above, and which operate in opposite directions. Whether they compensate or not it is impossible to say. The calculation, so far, assumes a uniform rate of supply since the surface of the earth took on its present form, and that assumption has been well criticized by Becker.[14] He shows that in all probability the rate is diminishing, for the reason that the exposure of fresh rocks, of unleached material, is constantly growing less and less, and the true age of the earth since stability was established, lies between 55 and 70 millions of years. The higher of these values appears to be the more probable. If, however, the ocean were primitively saline, the quotient representing its age would be still smaller.

Sodium tends to accumulate in the ocean, while the other saline radicles added to it are more or less precipitated as solid deposits on its floor. Calcium and magnesium are removed as carbonates, silica goes to build the skeletons of radiolarians, diatoms, and so forth; potassium is taken to produce glauconite, etc. These deposits or sediments cover vast areas to an unknown thickness, but their

[12] *Trans. Roy. Soc. Dublin* (2), Vol. 7, p. 23; Rep. British Assoc. Adv. Sci., 1900, p. 369.

[13] *Quar. Journ. Geol. Soc.*, Vol. 65, p. xli.

[14] Smithsonian Misc. Coll., Vol. 56, No. 6.

annual increment can be approximately determined. If, from the yearly contributions of rivers the amount of each radicle remaining in solution is subtracted, the rate of chemical sedimentation becomes known. In order to make this calculation, the age of the ocean must be assumed; but variations in the latter estimate affect the results but little. For example, the ocean contains 571.7×10^{12} tons of dissolved calcium, which, divided by the age, gives the annual addition. If the age of the ocean is 100,000,000 years the annual increment of calcium in solution is 5,717,000 tons; if only 50,000,000 years it is 11,434,000 tons. Subtracting these quantities from the total calcium of the river waters the remainders become 551,953,000 and 546,236,000 tons respectively, the difference being much less than the uncertainties in the data employed. If, for the sake of uniformity, we take the uncorrected age of the ocean, 83,472,000 years, the chemical or biotic sediments are represented by the following annual quantities.

SO_4	288,021,000	metric tons.
Ca	550,821,000	metric tons.
Mg	71,951,000	metric tons.
K	34,971,000	metric tons.
$(Al, Fe)_2O_3$	75,213,000	metric tons.
SiO_2	319,170,000	metric tons.

These are the quantities of the several substances annually removed from solution in the ocean, which, in combination assume the following form.

$CaCO_3$	1,347,440,000	metric tons.
$CaSO_4.2H_2O$	50,936,000	metric tons.
$MgCO_3$	251,830,000	metric tons.
K_2SiO_3	69,045,000	metric tons.
Limonite	87,905,000	metric tons.
Silica	295,096,000	metric tons.
Total	2,104,252,000	metric tons.

The last group of figures needs some explanation. From the analyses of oceanic sediments published in the reports of the Challenger Expedition I find that the ratio between sulphate calcium and carbonate calcium is 1:45.5. Calcium, therefore, is apportioned between the two salts in that ratio, but much of the SO_4 radicle is left

unaccounted for. Part of it goes to form pyrite, and part is decomposed by organic agencies and lost, but the proportion of loss is unknown. It is, doubtless, large. The potassium which is taken up by clays or else in glauconite is in either instance represented as silicate, and hence a part of the silica is regarded as in combination. The sesquioxides are calculated as limonite, although a part of them is certainly alumina; but no refinement of a calculation here would change the order of magnitude as given. The several orders of magnitude are probably close to the truth, and we may say with much confidence that the precipitates, including such substances as coral, shell, diatomaceous ooze and what not are formed at a rate of something like 21×10^8 metric tons a year, plus a small but undefined allowance for that part of the sulphur which has been fixed as pyrite.

At the figure given, chemical sediments are now forming in the ocean sufficient to cover 88,000,000 square miles of the sea floor to the depth of 0.0001337 inch annually. The whole area of the ocean is 139,440,000 square miles, but the portion covered by the red clay, where the precipitation is relatively insignificant, must be deducted. If the rate had been uniform throughout geological time, 83,472,000 years these sediments would form a layer about 930 feet deep, but such a calculation is unsound. Large areas of what were once marine sediments are now land, and, moreover, neither the rate nor the distribution of the deposits can have been uniform. The limestones that are forming now are largely derived from the solution of older deposits, Cambrian, Silurian, Devonian, Cretaceous, etc., and their carbonates have been deposited in the ocean, not once only, but possibly several times. In the earliest geologic eras, when sediments began to form, the proportion of carbonates to other salts thrown down must have been much smaller than today. An average thickness of 930 feet over the assumed area is therefore a great exaggeration; and needs to be reduced.

It is probably impossible to determine, with any approach to precision, the actual quantity of marine sediments that have been formed. We can, however, make a plausible estimate, which shall, at least, give us some conception of their order of magnitude. It has

already been shown that the limestones, which are mostly of marine origin, have a volume of 3,916,400 cubic miles. With a specific gravity of 2.7 their mass becomes $42{,}092 \times 10^{12}$ metric tons. From the figures given on p. 230 *ante*, the calcareous and magnesian sediments are now forming at a rate bearing a certain ratio to that of the other deposits, the limonitic and siliceous residues. This ratio, which is roundly 1,650:452, if constant throughout geologic time, would give for the latter class of sediments, proportional to the limestones, a mass of $11{,}664 \times 10^{12}$ tons; the sum of both classes of precipitates being $53{,}756 \times 10^{12}$ tons. The corresponding average thickness over the sedimentary oceanic area would then be 287 feet, or less than one third of the figure previously given. The actual thickness, however, must be much less; for a large part of the once marine sediments are now elevated into land. According to the best estimates,[15] the land area of the globe is now covered by 23 per cent. of archæan and eruptive rocks, and 77 per cent. of sedimentaries. Adding this sedimentary area to that of the ocean, the total becomes 132,180,000 square miles, and the average thickness of the chemical sediments reduces to 191 feet. At the crude value assigned to geologic time this represents a rate of deposition of only 0.000027 inch annually. If the age of the earth is less than 83,472,000 years, the mean annual rate of deposition will be proportionately increased, but not to anything like the present magnitude.

Whether the ratio assumed between the calcareous and siliceous sediments is justifiable or not, is a question admitting of argument. It seems, however, probable, that in the earliest geologic ages, when the land area was occupied principally by igneous rocks, the salinity of the rivers was relatively low, but the proportion of silica to lime in the waters was higher. This suspicion is justified by a study of the river waters of today, especially those issuing from granitoid areas. In such waters silica is often in excess of lime, while in waters from sedimentary areas the reverse is commonly true. The ratio here assumed represents a balancing between waters of both classes, and is therefore as legitimate as any other which might be chosen. Here it must be borne in mind that we are dealing with probabilities only, nothing more.

[15] Von Tillo as modified by Becker. See Becker's memoir already cited.

So far, the mechanical sediments, such as silt and sand, have not been considered. From the surface of the United States, according to Dole and Stabler,[16] the rivers annually carry to the sea 270,000,000 tons of dissolved substances, and 513,000,000 tons in suspension. If this ratio, which is only approximate, should hold for the whole world, the quantity deposited in the ocean during geologic time would be $102{,}370 \times 10^{12}$ tons, and the total sedimentation, chemical and mechanical, becomes $156{,}126 \times 10^{12}$ tons. This quantity, distributed over the entire sedimentary area, continental and oceanic, gives an average thickness of about 550 feet, or 0.000079 inch a year.

The total volume of the marine sediments thus computed, is 13,873,000 cubic miles. The volume remaining in the ocean is very nearly two thirds of this figure, 9,239,000 cubic miles. The volume of all the secondary rocks derived from the decomposition of igneous rocks was previously found to be 78,338,000 cubic miles. Hence the portion now on the land area of the globe amounts to 69,099,000 cubic miles of rock, consisting in great part of materials which were never transported very far from their original place of formation.

To the foregoing estimates of the oceanic sediments at least one large but undetermined correction needs to be applied. The ocean receives great quantities of dust, representative of aerial erosion, and also quantities of volcanic ejectamenta. For these nonfluviatile additions no valid estimates can yet be made. The major portion of them, however, must come from disintegrated sedimentary rocks, sands, and soils, and so do not affect to any serious extent our estimates of rock decomposition. The oceanic share of the sediments should be increased, but less than appears at a first glance. The marine sediments now on land must include a part of the contributions made to the ocean by atmospheric transportation. The actual distribution of the sediments is naturally very uneven. They are probably thin near the margin of the red clay, and thick along the continental shelves. Coral rock, for example, has been bored to a depth of 1,100 feet without reaching its limit. The mechanical sediments are of course mainly deposited relatively near to shore.

[16] U. S. Geol. Surv. Water Supply Paper, No. 234, p. 83.

In order to prevent misapprehension it may be well to reiterate the statement that these estimates of the marine sediments are necessarily crude, and represent orders of magnitude only. With better evidence, better estimates may at some future time be made, but accuracy in them is unattainable. The other figures given, for the composition of the igneous rocks and of the ocean are probably near the truth but are still subject to revision and improvement.

THERMAL RELATIONS OF SOLUTIONS.

By WILLIAM FRANCIS MAGIE.

(*Read April 20, 1912.*)

The thermal relations of solutions afford evidence of a peculiar and valuable kind about the nature of solutions. The electrical conductivity of solutions has been explained by the hypothesis that the molecules of the solute are partly dissociated into ions in the solution. When this hypothesis is further tested on the assumption that the osmotic pressure is proportional to the number of free molecules and ions in the solution, the experimental results of Dieterici, of Kahlenberg, of Jones and others show disaccord with the predictions of the hypothesis. I venture to believe that a study of the heat capacities and the heats of dilution of solutions will confirm the view that the reason for this disaccord lies in the assumed relation of the osmotic pressure to the dissociation, so that while the dissociation hypothesis is confirmed, the relation of the osmotic pressure to the dissociation is shown to be different from that which was originally assumed.

Five years ago I presented to this Society a paper in which I discussed the heat capacities of solutions. I will summarize here the principal results described in that paper, in order to render the present discussion more complete.

The heat capacity of a gram-molecular solution of an electrolyte diminishes with increasing dilution. The change in the heat capacity is directly proportional to the change in the dissociation, determined from the electrical conductivity. In most cases, at ordinary concentrations, the heat capacity of the water is diminished, and we are led to infer an interaction between the water and the molecules and ions of the solute of such a sort as to diminish the freedom of the water. A study of the constants of the formula by which the heat capacity is expressed leads to the conclusion that the heat capacity of the

water associated with the molecules of the solute is increased, while that of the water associated with the ions is diminished. A similar relation holds for the volumes of solutions of electrolytes. Since the heat capacity and the volume of a body are both quantities in their nature additive of the similar properties of the parts of the body, it is to be expected that there will be a similarity in their behavior in any complex and changing body like a solution. The relations described confirm in a striking way the correctness of the dissociation hypothesis and the general accuracy of the dissociation factors obtained from observations on electrical conductivity, while they also indicate as a necessary conclusion from the facts, that there is interaction between the solute and solvent. This general theory which was formerly based on the facts which have been described, receives, I believe, strong confirmation from the study of the heats of dilution.

When a solution containing one gram-molecule of solute is diluted by the addition of a unit volume of water, heat is either evolved or absorbed. This amount of heat is the heat of dilution. It is of course a function of the concentration. It also shows a remarkable dependence on the temperature. For example, if a solution of barium chloride containing one half gram-molecule in 50 gram-molecules of water is diluted to double its volume at 7° C. 49 gram-calories of heat are absorbed. The same dilution at 24.5° C. is accompanied by an evolution of 18.5 gram-calories. At some intermediate temperature, about 17° C., there is no heat either evolved or absorbed. In all cases the absorption of heat is greatest at the lowest temperature of observation, and diminishes as the temperature is raised, to become positive in some cases, as in the example just given, at temperatures common in an ordinary room. The greater the dilution, the lower the temperature at which the heat of dilution changes its sign.

A very simple thermodynamic argument proves that the rate of change of the heat of dilution with the temperature, for a fixed concentration, or what we may call the temperature coefficient of the heat of dilution, is equal to the negative value of the rate at which the heat capacity of the solution changes with an increase of volume of the solution. Using l to represent the heat of dilution, taken positive if heat is evolved, θ, the absolute temperature, H, the heat

capacity of the solution and of an arbitrary amount of additional solvent, from which the solvent is taken with which the solution is diluted, and v, the volume of the solution, we find

$$\frac{dl}{d\theta} = -\frac{dH}{dv} = -a.$$

We may set $dH/dv = a$, a quantity independent of the temperature, because, first, Teudt has proven it to be so independent, within reasonable ranges of temperature, by direct observations, and, secondly, because the formula

$$l_1 - l_2 = -a(\theta_1 - \theta_2)$$

obtained from the differential equation on that supposition agrees with observations within the temperature ranges in which it has been tested. Treating a therefore as independent of the temperature we get

$$l = -a\theta + e,$$

in which the heat of dilution is expressed as the sum of two terms, one of which is proportional to the absolute temperature, the other independent of temperature. Such a relation could not be expected to hold for all temperatures, but within the narrow ranges open to experiment it seems to be valid. Of the two terms the first one is positive in all actual cases, for it is a general rule that the heat capacity of an electrolyte diminishes as the dilution increases. It corresponds to an evolution of heat. The second term is nearly of the same magnitude as the first, and is negative, corresponding to an absorption of heat.

From the experimental relation already described, connecting the heat capacity of the solution with the dissociation, it follows that the quantity a is equal to a negative constant multiplied by the rate at which the dissociation increases as the volume of solution is increased. The evolution of heat therefore which is expressed by the term $-a\theta$ is proportional to the increase in the dissociation and to the absolute temperature. This can be explained by the theory of the constitution of a solution which has already been described. As dissociation proceeds molecules of water which have been in union

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with the molecules of the solute are released and their heat capacity is diminished. At the same time molecules of water are associated with the new ions, and their heat capacity is also diminished. The act of dissociation thus diminishes the heat capacity, and therefore the number of degrees of freedom of the solution in proportion to the change of the dissociation factor, and the energy associated with those degrees of freedom, which is proportional at these temperatures to the absolute temperature, is liberated in the solution as heat. The negative term e being nearly equal at each concentration to the corresponding positive term, also varies to a first approximation with the dissociation. When calculated for the dissociation of a complete gram-molecule it is of the same order of magnitude as the heat of combination of the elements constituting the solute. It presumably contains the heat of dissociation along with the internal work done during the dilution.

The condition that $dH/dv = a$ is independent of the temperature enables us to find a formula for the relation between the osmotic pressure and the temperature expressed in terms of the thermal constants. A thermodynamic argument shows that the osmotic pressure p satisfies the differential equation

$$\frac{d^2p}{d\theta^2} = -\frac{a}{\theta};$$

and if a is assumed independent of θ we integrate this equation and obtain

$$p = -a\theta(\log\theta - 1) + b\theta + e.$$

The quantities b and e are functions of the volume but are independent of the temperature.

Now by another thermodynamic argument we can deduce the heat of dilution from this value of the osmotic pressure. We obtain

$$l = -a\theta + e$$

as before, in which the function e is that which appears in the expression for the osmotic pressure. This formula can be tested by comparison with the results of experiment. From observations of the depression of the freezing point we can calculate the osmotic pressure for a fixed concentration at 0° C. and using this in con-

nection with the heat of dilution for the same concentration we can determine the constants of the formula. With these we can then calculate the osmotic pressure at 100° C. and compare it with the value found for this quantity from observations of the elevation of the boiling point. I was able to do this with the observations of Kahlenberg on the freezing and boiling points of sodium chloride solutions combined with those of Thomsen on the heat capacities and of myself on the heats of dilution. The agreement of the observed boiling points with those predicted from the formula was excellent. Incidentally this agreement confirms the validity of the assumption from which the formula was derived, that a is independent of the temperature.

Another test of a less searching character can be made by using the osmotic pressures given by the formula at different temperatures to calculate the ratio of the vapor pressure of the solution to that of the pure solvent. According to von Babo's law this ratio should be independent of the temperature. Calculations for sodium chloride solutions show that while it is not strictly the same at all temperatures between 0° and 100° C. yet the differences between ratios at different temperatures are excessively small, and lie within the errors of the observations by which von Babo's law has been tested.

The terms a and e of the formula for the heat of dilution are manifestly not quantities which are fundamentally kinetic in their nature. They express rates of change of energy with change of volume. Their appearance in the formula for the osmotic pressure indicates that the osmotic pressure is not to be explained as a kinetic phenomenon, as the pressure of a gas is, but as the result of forces acting between the solute—its molecules and ions—and the solvent. These thermal relations, therefore, afford strong evidence, and evidence with as little admixture of hypothesis as is possible in the nature of the case, first of the validity of the dissociation hypothesis by which the laws of electrolytic conduction are explained, and secondly, of the dependence of the osmotic pressure on the forces which are exerted between the parts of the solution and the pure solvent.

PALMER PHYSICAL LABORATORY,
PRINCETON UNIVERSITY.

NEW MAGNETIC CHARTS OF THE INDIAN OCEAN.

By L. A. BAUER.

(*Read April 20, 1912.*)

The charts exhibited embody the results of magnetic observations made during the summer and fall of 1911 on board the non-magnetic yacht *Carnegie* operating under my direction as Director of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington.

The necessity of the new charts arose from the exceptionally large errors found in the most recent magnetic charts at present in use by mariners. Thus, for example, the errors in the compass directions for two of the most recent charts approximate respectively four degrees and six degrees, though one of them was issued as recently as 1910. With the exception of a few values found by the vessel, the *Galilee,* used in the Pacific Ocean work, these are the largest errors thus far revealed. In the portions of the Atlantic Ocean thus far covered by the *Carnegie* the compass chart errors have generally been below two degrees, though running at times up to two and a half degrees.

The chart errors in the compass directions are usually found to be systematic, that is, in the same direction for large stretches, and are to be ascribed largely to erroneous secular changes allowed for in attempting to bring previously observed values up to date.

Thus, for example, by comparing the *Carnegie* values of 1911 with those obtained on board the German Antarctic vessel, the *Gauss,* in 1903, it is found that the north end of the compass moved to the eastward (hence diminished west declination) at the average rate of about 11′ per year off the southeast end of Africa, whereas in the vicinity of the islands of St. Paul and New Amsterdam in the Indian Ocean (lat. 35° 16′ S., long. 74° 46′ E.) it moved to the westward (increased west declination) at the average rate of about

13′ per year. The charts give secular changes of only about one fourth of these amounts, so that the error of reduction in but ten years amounts to almost 2°. It is doubtless due to these large secular changes disclosed in the Indian Ocean, and especially their rapid variation with geographic position, that the large errors mentioned have crept into the charts.

The errors in the other magnetic elements, while of less importance to the mariner, are of consequence to theoretical investigations regarding the earth's magnetism. In the magnetic dip, the errors on the present cruise have amounted at times to 4°, and in the horizontal intensity to about one-twentieth part. While some of the results derived from previous analyses of the earth's magnetic field have pointed to the possibility of large and more or less systematic chart errors, it was not suspected that they would reach the magnitude disclosed by the work of the *Galilee* and of the *Carnegie*.

The *Carnegie* is at present making a circumnavigation cruise and is expected back in New York towards the end of 1913, having left the same port in June, 1910. Up to February 1, 1912, this vessel had already covered about fifty thousand miles. She left Manila on March 23, in command of Mr. W. J. Peters, bound for the Fiji Islands.

Owing to the non-magnetic structure of the *Carnegie* and the absence in consequence of any deviation corrections, it is possible to obtain and communicate results expeditiously. The data are promptly transmitted to the chief hydrographic establishments issuing magnetic charts in order to enable them to make the necessary corrections from time to time.

THE DIARY OF A VOYAGE TO THE UNITED STATES, BY MOREAU DE SAINT-MERY.

By STEWART L. MIMS.

(*Read April 18, 1912.*)

In his *Souvenirs intimes sur Talleyrand,* published at Paris in 1870, M. Amédée Pichot remarked in his preface:

> "If we were to write a complete biography of Talleyrand, we would be able to give some details, very little known, concerning his exile in America, where M. de Beaumetz and he found themselves with other notable *émigrés* among whom was Moreau de Saint-Méry. . . . This information has been obtained from an unpublished diary, kept by Moreau de Saint-Méry, which M. Margry has examined and from which he has communicated to us certain extracts concerning the sojourn of Talleyrand at New York, Boston and Philadelphia."

Pichot made two quotations from this "unpublished diary," one at pp. 209–212, describing the intimate relations existing between Talleyrand and Moreau, the other at pp. 212–213 giving the text of a letter written by Talleyrand to the French Minister of Foreign Affairs to acknowledge a letter inclosing the decree of September 3, 1795, which reopened the doors of France to the famous exile. Pichot contented himself with these two citations, either because his friend Margry[1] did not choose to give him more material, or because the limitations of his own study did not permit him to quote more extensively from the notes which were actually communicated to him.

Although many studies have appeared on the life of Talleyrand since 1870 and some have made use of Pichot's citations, apparently none, not even such recent biographers as MacCabe, Lacombe and

[1] Pierre Margry, author of "Mémoires et Documents pour servir à l'histoire des origines françaises des pays d'outre-mer" and of some studies relating to the history of French colonization in America, was at this time archivist at the Ministry of the Marine.

Loliée,[2] have attempted to find the source from which the citations were taken to see whether it contained other interesting material upon the sojourn of the great diplomat in America.

It was my good fortune, a little over a year ago, to find in the manuscript catalogue of the *Archives Coloniales* at Paris, the title, "Le Voyage aux Etats-Unis de l'Amérique par Moreau de Saint-Méry pendant les années de 1793 à 1798."

Although it called me far afield from the work in which I was engaged, I could not resist the temptation to cast a furtive glance at the manuscript to see what its interesting title meant. That furtive glance grew into the absorbing task of reading from page to page until I had finished the story which the volume contained—a story all but forgotten and lost for three generations among the dusty archives of the Colonial Office.[3] It is to this story that I wish to direct your attention for a few moments.

With the author of the diary many of you are already acquainted from the paper which one of your members presented before this society at its last annual meeting.[4] Permit me to recall, however, the salient facts in his life.

Born at Fort Royal, Martinique, on January 13, 1750, Médéric-Louis-Elie Moreau de Saint-Méry[5] was of a family which had emigrated from Poitou to settle at Martinique in the seventeenth century and had won a place of prominence by furnishing in succeeding

[2] MacCabe, "Talleyrand, a Biographical Study," London, 1906; Bernard de Lacombe, "La vie intime de Talleyrand," Paris, 1910; Frédéric Loliée. "Talleyrand et la société française," Paris, 1910.

[3] Victor Tantet, late archivist of the *Archives Coloniales,* made use of the diary to write a very interesting article which appeared in *La Revue* (1905), Vol. 52, pp. 378–396, and entitled "Les Réfugiés politiques français en Amérique sous la Convention—Moreau de Saint-Méry libraire à Philadelphie." I knew nothing of the existence of this article until after I had found and studied the diary.

[4] Joseph G. Rosengarten, "Moreau de Saint-Méry and his French Friends in the American Philosophical Society," *Proceedings* of this Society, Vol. 50, pp. 168–182.

[5] This short sketch of Moreau is based on Silvestre, "Notice Biographique sur M. Moreau de Saint-Méry," Paris, 1819 (a short pamphlet of 24 pp.), and Fournier-Pescay's article in *Biographie Universelle* on Moreau de Saint-Méry.

generations many judges to the principal courts of the island. After spending his boyhood days at Martinique, the young Moreau, at the age of nineteen, took ship for France to acquire that training in the principles of the law which would fit him to follow in the footsteps of his fathers.

After his arrival at Paris, he began to study with such rare enthusiasm and success that at the end of fourteen months he sustained his thesis in Latin and received the degree of bachelor of law. At the end of three years he won the honor of attaining the rank of *avocat au parlement* and was prepared to turn his face towards his native island in order to follow his chosen profession.

At his return to Martinique he found himself an orphan, the fortune of the family in ruins and nothing to rely upon, in making a place for himself in the world, except the training which he had just received. The French colony of St. Domingo, the richest of all the West India islands, seemed to offer a more promising future and accordingly he left Martinique and settled at Cap Français (today Cape Haiti) in 1772 to begin the practice of law. After eight years of successful practice, he was elected a member of the *conseil supérieur* of St. Domingo. It was in the discharge of the duties of this office that Moreau began the difficult task of codifying the laws of the island in order that his decision as a judge might be more intelligent and scientific.

The previous attempts which had been made to codify these laws in 1716, 1738 and 1757 had not been successful. The King had commissioned de la Rivière in 1771 to undertake the task, but his work had been slow and discouraging. Hearing that Moreau was engaged in the same work, de la Rivière gracefully gave way to the younger and more competent man. In preparation for his work Moreau visited all parts of St. Domingo, Martinique, Guadeloupe and St. Lucia, thus laying the basis for that larger and later work of preparing his monumental collection of documents, relating to the history of the French West Indies and to be found today in the *Archives Coloniales* at Paris—a collection which has made his name immortal among all students of West India history. He received a commission from Louis XVI. to return to Paris in order to com-

plete and publish the results of his work. From 1784 to 1790 appeared successively the six volumes of his well-known collection of laws, entitled "Loix et Constitutions des Colonies françaises de l'Amérique sous le Vent."

At his return to Paris, Moreau quickly won admission into the intellectual and political life of the capital. He became one of the founders of the Musée de Paris and contributed much to its efficiency. At the outbreak of the revolution in 1789, he became one of its ardent champions. He was chosen as one of the electors of Paris, who at one time virtually ruled over all France. He boasted afterwards during his exile that in serving as their president he was "king of Paris for three days." He was deputy of Martinique in the *Constituante,* and played an important part, especially in matters which related to the administration of the colonies.

But with Robespierre's accession to power and the inauguration of the Reign of Terror in 1793, Moreau was forced to flee from Paris and seek refuge in Normandy. It was only a temporary refuge that he found, however, for Robespierre placed his name upon the fatal list of those for whose blood he thirsted. Gathering up all that he held most precious, among them the manuscripts of some unpublished works, Moreau escaped with his family to Havre, where by good fortune a ship was ready to sail for the new world.

It was therefore in quality of an *émigré* that Moreau de Saint-Méry set out upon a voyage to the United States. He sailed from Havre on November 9, 1793, aboard the *Sophie* (Lowther, Captain) bound for New York. After a long struggle against head winds and frequent storms, lasting for one hundred and nineteen days the vessel was forced to put in at Norfolk, where it cast anchor on March 8, 1794.[6] After a stay of two months at Norfolk, Moreau proceeded by water to Baltimore, passing thence to New York by way of Wilmington, Philadelphia and Princeton. He remained at New York from May 25 to August 21, being forced to earn his living by hard work as shipping clerk for the house of Daniel Merian, which was only a *"prête-nom"* to conceal the shipment of provisions to the French government. During his residence in New York, Moreau

[6] The diary contains a most interesting account of this voyage.

made the acquaintance of a German nobleman, de la Roche by name, with whom he formed a partnership for the establishment at Philadelphia of a stationery and book-store and printing shop. He accordingly removed to Philadelphia, where he remained from October 14, 1794, to August 23, 1798, when he set sail for France.

It is the story of these five years in America that Moreau de Saint-Méry records in his diary. Step by step one may follow him to hear his appreciation of the hospitality of old Virginia or his enthusiasm over the beauty and charm of the Virginia belle, whose musical voice seems to have fired his heart; or to hear him marvel at the number of religious sects at Baltimore, all living in the perfect harmony of an attractive society, or one may turn to the descriptions of the life of New York and Philadelphia. In these descriptions and throughout the diary are to be found many interesting data on the prices, the cost of living, the expenses of travel, the manners and customs of the people. In other passages one may follow the author into the presence of such public men as Washington, John Adams or Alexander Hamilton. Thus under date of August 26, 1794, is recorded a visit to the last named:

"I went to see the Minister, Hamilton, having a letter of introduction from Talleyrand. On arriving at the building in which his office was located, I found a porter, clad in a long gray vest, who informed me that the minister was not in. On leaving the house and before I had gone very far, I met a gentleman whom I believed, tho' I know not why, to be Hamilton and I turned and followed him. We entered the corridor almost together. On seeing Mr. Hamilton, for it was he, the porter took a key down from a peg and opened the door of the Minister's office and I was asked to enter. I was much pleased that I had been warned not to have the air of being inquisitive or of plying the minister with questions. But he showed great confidence in me by talking frankly of both France and America. . . . I did not fail to be struck by his bureau. Its furniture and furnishings together were not worth more than fifty francs [ten dollars]. A large pine table, covered with a green cloth, served as his desk. His books and papers were upon simple wooden shelves. On the end of one shelf was a slate-colored vase and a plate with some drinking glasses. The porter who waited on him wore, besides the vest of which I have spoken, trousers of the same cloth which did not reach low enough to hide his bare leg. In a word I saw around me what must have been in accord with the customs of the Spartans."

Moreau's later impressions led him to speak of Hamilton as "devoured with the ambition of making people believe that he was

the soul of everything" [in the government] and that "the security of the United States depended entirely upon his ability." Again he wrote:

"Hamilton, who knew only America and was unacquainted with Europe except through books, was a lover of a strong centralized government which knew how to command obedience without compromise. Civil war did not frighten him, because he was a man of courage and had natural military ability and believed and in fact was accustomed to say that the United States would never have a real and stable government until internal dissensions had proved its necessity and caused it to be established. . . . He was of small stature, had an admirable composure, very small eyes and had something of the mysterious in his countenance. He spoke French, but very imperfectly. He was a man of much intelligence and kept a close watch upon himself. He was very brave, disregarded personal interests in his public service, was an admirer of the laws and of the government and financial system of England. He was very dictatorial and was very jealous of the prerogatives of the executive power."

Besides these interesting features, the diary has another of perhaps greater value, namely, some remarkable passages which describe the group of French *émigrés* at Philadelphia and the intimate relations existing between some of the chief among them.

It has already been noted that Moreau de Saint-Méry established a book-store and printing press at Philadelphia. His house was located at the corner of Front and Walnut streets. From the catalogue which he published at the beginning of his career as bookseller,[7] it can be seen that his store offered for sale many books printed in English, Dutch, Italian, Spanish and French—a fact which must have attracted not only cultured Philadelphians, but also the French *émigrés* in the city. His store became in fact the rendezvous of many notable exiles from France. Talleyrand, de Noailles, Talon, de Beaumetz, Demeunier, La Colombe, La Rochefoucauld, duc de Liancourt, and the duc d'Orléans (the future Louis Philippe) were all visitors at the shop of Moreau. Some of them, including Talleyrand, Blacon and de Beaumetz, frequently remained for supper and like boys scuffled and played pranks upon one another about the store.

In regard to Talleyrand the most notable of the many *émigrés*

[7] A copy of this catalogue is still to be found in the library of the American Philosophical Society.

who found a temporary refuge in "the ark of Noah," as Philadelphia was happily called by one of them, the diary contains some important data. Moreau's first meeting with Talleyrand in America is recorded under the date of May 22:

"After the end of the session of Congress, I was on my way with my two companions [Goynard and his son] to see the executive mansion which was in the course of construction, . . . when I noticed that in a stage which approached rapidly there were two men waving their hands at us. . . . One of them jumped to the ground and ran to throw himself in my arms. It was Beaumetz. The other, less agile, climbed down from the stage. It proved to be Talleyrand. Both of them had just arrived from England. What joy! What happiness! How many repeated embraces! . . . They invited me to dine with them. I went at once to break the good news to my home and then rejoined them. What a glorious dinner it was! How many things we had to tell one another after two years of separation! After dinner we all went to see Blacon, the comte de Noailles and Talon. New surprises and new rejoicings! During our reunion a hail and thunder storm raged outside as though heaven wished to recall to our minds the misfortunes from which we had escaped in our own country."

From this date forward it is easy to trace in the diary the intimate relations which were established between Moreau and Talleyrand. Here is an entry to tell us that they have dined together, there, another to describe an evening together, or yet another to describe a little group with Talleyrand in their midst to listen to Moreau read from the yet unpublished manuscript of "La Danse" or of his description of St. Domingo. When the two were separated, the diary indicates a constant exchange of notes and of letters. In regard to this, it may be interesting to note in passing, the diary contains no less than seventeen notes and letters from the hand of Talleyrand to his "*cher maître,*" as he came to call Moreau in affectionate terms. When Talleyrand was at Philadelphia their relations became most intimate. Moreau's own words must tell the story:

"No words could express adequately the nature of my liaison with Talleyrand. Every day, after his return to Philadelphia in the month of October, 1795 to June 11, 1796 [the date of Talleyrand's departure for Europe], he came to my office at eight in the evening. There alone (except when Beaumetz, Talon, Blacon, de Noailles, Volney or some others came also) we opened our hearts to one another and shared one another's deepest feelings. We told our most intimate secrets. Thus we passed our time

together in delightful communion until supper was announced. Talleyrand ordinarily did not sup, while I ate some rice cooked with milk on the stove in my store. . . . I had some excellent Madeira which Talleyrand liked very much. . . . How many times, after the late hour had dispersed the rest of the company, did Talleyrand go with them across the little court-yard and then steal back to prolong the evening with me. He yielded finally when my wife came and said to him: 'Tomorrow you will stay lazily in your bed until noon, whereas your friend must be up and open his shop at seven.' . . . Thus we passed every evening together without missing a single one, in talking of the past, of the present and of the future of our country. In connection with the future we talked of Louisiana and of plans to colonize it for ourselves. Sometimes we talked seriously of the matter and Talleyrand concluded that we must become the governors.

"In this plan, as well as in others which we made to remain together, Talleyrand and I closed our talks together, our hands clasped in a pledge that for the rest of our lives we would share with one another our failures and our successes even in money matters. . . . In a word never did the common expression, 'united as two fingers of the same hand,' describe so accurately the liaison between two persons as that between Talleyrand and myself."

At his departure for Europe on June 11, 1796, Talleyrand took with him some two hundred copies of Moreau's "La description de la partie espagnole de St. Domingue" to find sale for them at Hamburg and in France. He offered to take Moreau's son with him back to Paris and to provide for his education. After his return to Europe Talleyrand did not forget the friend of his days of exile, for the diary contains letters written in affectionate terms from Hamburg and Paris. It was in fact through Talleyrand that Moreau was permitted to return to France and by Talleyrand's pecuniary aid that he was able to take his family from Bordeaux to the capital. It was due to Talleyrand's influence that Moreau obtained shortly afterwards the position of historiographer at the Ministry of the Marine.

The diary closes at a date shortly after Moreau's return to Paris and we are left to conjecture as to what were the relations of the two friends in later life. It seems, however, perfectly plausible to suppose that the appointment of Moreau as ambassodar at Parma in 1801 (later to become and remain its regent until his disgrace by Napoleon in 1806) was the result of Talleyrand's influence exerted in his behalf.

It is pleasant to dwell upon these pages of the diary, because they throw a pleasing light upon Talleyrand's character. He is generally thought of as the prince of diplomats, employing human speech to conceal his own thoughts, but here we have him unveiling his very soul to a kind and sympathetic friend.

The diary shows, however, that Moreau de Saint-Méry's shop became something more than the rendezvous for such notable *émigrés*. From his printing press went forth many notable works published in French, such as de Liancourt's study on the prisons of Philadelphia, and three of his own notable works, "La Danse," "La déscription de la partie espagnole de St. Domingue" and "La déscription de la parte française de St. Domingue."[8] At his press also was published from October 15, 1795, to March 14, 1796, a daily newspaper in French entitled *Courrier de la France et des Colonies*,[9] edited by Gaterau, an *émigré* from St. Domingo. In its pages were printed the latest news of the great revolution in France and of the most recent developments in the French West Indies. How eagerly the numerous *émigrés* then at Philadelphia and in other cities must have read it! It must have come as a messenger to them in their exile. It served also as a social organ for them, for it contained notices of balls and concerts and meetings of French societies. Moreau's press, therefore, in publishing such a paper must have occupied an important place in the lives of the wider circle of French exiles in Philadelphia. The diary contains passages which throw light upon the life of this wider circle and reveal something of its numbers and of its importance.

There is to be found in the diary, therefore, a wide range of material, varying from passing comments upon public men and upon the customs of the people to serious studies upon the history and life of some of the chief American cities, and including some new and most interesting material upon the life of many exiles who fled from persecutions in France and revolts in the West Indies to find refuge in our fair land of liberty and freedom.

[8] These three works appeared at Philadelphia during the years 1795–1798.

[9] A complete file of this unique publication is to be found in the library of the Athenæum at Boston. It appeared in a single sheet, 4 pages, each page measuring 24½ c.c. by 20 c.c.

In closing, I should like to express my thanks for this opportunity of calling the attention of the American Philosophical Society to this unpublished diary of one of its former members,[10] who must have spent, as indeed its proceedings show, many delightful evenings in its halls and who with pride placed after his name upon the title pages of his well-known works "Member of the American Philosophical Society."

When he sailed upon the voyage which was to take him back to the country and to the people he loved, Moreau de Saint-Méry must have left in this society many friends who thoroughly appreciated his great talents and who had been attracted by his interesting personality. It is fortunate that such a man left for us a record of his sojourn in America and that it is possible to rescue it from the dust of archives.[11]

[10] Moreau was elected to the society on January 16, 1789, before he left France. The records show that he attended its meetings regularly after his arrival in Philadelphia in the fall of 1794.

[11] The diary will be published in the near future at the Yale University Press.

THE CLASSIFICATION OF CARBON COMPOUNDS.

By MARSTON TAYLOR BOGERT.

(*Read April 20, 1912.*)

The system of classification adopted for a science at any given period registers quite accurately the state of the science at that period, and the changes in the classification therefore record its progress. It is, hence, practically impossible to give any intelligible description of the various methods of classification which have been employed for carbon compounds without at the same time sketching briefly the changing conceptions and theories of which they were to so large an extent the natural reflection, for without such a setting the picture would have no proper background or perspective.

The classifications which are considered are particularly those which have been used for textbook instruction in organic chemistry, and no place is given to those which have been devised solely for the patent offices, for reference, or for other special purposes.

Man being naturally of an inquiring mind, he has probably speculated upon the composition of this world of ours ever since he first appeared upon it, for in the oldest records we find theories concerning the elements of which it is composed.

The doctrine of the four so-called "elements"—earth, air, fire and water—was first enunciated in Greece by Empedocles, about 440 B. C., but generally bears the name of Aristotle. Neither Empedocles nor Aristotle regarded these elements as different forms of matter, but rather as different properties or manifestations of one original matter. Aristotle also added a fifth element, *οὐσία*, to which he ascribed an ethereal or immaterial character and which he assumed permeated the universe. As the oldest writings of India contain a similar theory of four elementary principles and an ethereal substance, it is possible that both Aristotle and Empedocles

were familiar with this fact and were only introducing into Greece this ancient Indian theory.

The oldest nations were familiar with the metals and refer to them frequently in their writings, but it should not be forgotten that some of the earliest chemical facts on record have to do with carbon compounds. The only acid known to the ancients was acetic (as vinegar), so that the name of this substance and the idea of acidity were expressed by closely related words; in the Greek, ὄξος for vinegar, and ὀξύς for acid; in the Latin, acetus and acidus. The first reagent of any kind mentioned was the extract of gall nuts, which Pliny says the ancients used to detect the presence of green vitriol in verdigris. The first salts artificially prepared were those obtained by the action of vinegar upon alkalies. The first crude attempts at distillation were with turpentine. The ancients were familiar also with fats, resins, organic coloring matters (like indigo and Tyrian purple), sugar, gums, the preparation of wine from grape juice, of beer from malted grain, of mead from honey, of soap from fats, and many other facts in these and related fields. Organic chemistry, therefore, does not give place in point of age to inorganic. Largely due to the influence of Alchemy, however, the object of which was the transmutation of baser metals into silver and gold, the mineral side of the subject was the first to be extensively developed.

According to the pseudo-Geber, all metals consisted of sulfur and mercury, in varying amounts and in different degrees of purity. The old Aristotelian "elements" he appears to have regarded as subsidiary constituents, or perhaps as the ultimate components of the sulfur and mercury. To the pseudo-Geber's two elements, Basil Valentine added a third, "salt," not meaning any particular compound but the properties characteristic of common sodium chloride, and he assumed these three to be the elementary constituents not only of metallic substances but of organic as well; sulfur endowing the substance with combustibility, or the property of changing in the fire, and also explaining color changes, mercury giving metallic properties and volatility, and salt representing the principle of solidification and of resistance to fire.

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In spite of the great amount of experimental work carried out by the alchemists, and the large number of new facts discovered by them, their writings were so obscured by mysticism, exaggeration and deceit, that little real progress was made toward a more accurate understanding of the nature of chemical compounds which might be utilized in constructing a more satisfactory method of classification. No attempts were made to determine the actual constituents of compounds, for it was assumed that in the formation of a compound the original substances were annihilated and an entirely new substance created. Hence the only classification in vogue was a rough grouping of substances according to their physical properties, or apparent outward resemblance, and many of our common names are reminders of this bygone empirical method. Thus, olive oil and other vegetable and animal oils were grouped with oil of vitriol and oleum tartari (deliquesced potassium carbonate); spirit of wine (alcohol) with fuming spirit of Libavius (stannic chloride), spirit of hartshorn (ammonium hydroxide solution) and spirit of nitre (nitric acid); butter with butter of antimony (antimony trichloride) and other semi-solid metallic chlorides. Colorless solids, soluble in water and of characteristic well marked taste, were all classed as "salts," and this group thus included sugar.

The goal toward which the alchemists strove was the philosopher's stone, the grand elixir or the magisterium, as it was variously called, whose virtues were such that it could not only transmute baser metals into silver and gold, but could also prolong life indefinitely. As the claims concerning the transmutation of metals were increasingly discredited and the trickery and deception of the alchemists exposed, more investigators directed their attention toward the second great function of the philosopher's stone, the prolongation of life, and many compounds were discovered of considerable therapeutic value. Great interest was aroused by these investigations, and Paracelsus finally announced that "the object of chemistry is not to make gold but to prepare medicines." Thus, in the first half of the sixteenth century, chemistry began to develop in a new direction, at first not far removed from alchemy, but gradually diverging from it more and more widely, and approaching closer and closer

to medicine, until the coalescence of the two sciences appeared practically complete. And thus arose the period of iatro chemistry, when chemistry, which had long been looked upon as a valuable helpmeet to medicine, came to be regarded as the basis of the entire medical art.

Although in this period the chief development was again along the mineral side, probably because of the relatively greater simplicity and stability of these preparations, still no little organic investigation was conducted and a number of new compounds were added to the science. Little progress was made in gaining a truer insight into the character of chemical compounds, and hence no important changes in classification appear. Paracelsus himself, the founder of the iatro-chemical school, adopted Basil Valentine's three elements (sulfur, mercury and salt) as the basis of his doctrines.

By the middle of the seventeenth century, chemistry awakened to the fact that it had a destiny of its own to realize, struggled to its feet and, refusing longer to be supported by other sciences, started forward, to be sure rather unsteadily and uncertainly at first, but with the firm determination to do something for itself.

The history of chemistry proper begins with Robert Boyle about 1660, who taught that its main object was the determination of the composition of matter. Through his labors, and those of Rouelle and others, the terms "element" and "chemical compound" were more fully explained and appreciated; nevertheless many of their colleagues still adhered to the old alchemical or even the Aristotelian elements. Kopp, in his "Geschichte der Chemie," gives an excellent picture of the epoch-marking effect of Boyle's ideas:

> "What a contrast is exhibited between the ancient idea of the cause of difference in various forms of matter and that which obtained at the time of Boyle! If we consider these two opposite conceptions historically, and the transition from the one to the other, they appear like two totally dissimilar pictures; but, like dissolving views, changing the one into the other by slow degrees. In the first place we have the Aristotelian idea, according to which, matter itself devoid of properties, becomes endowed with characteristic qualities by the addition of properties, and forms, when invested with these properties, the various substances known in nature; then this idea passes gradually into that of the alchemists, but becomes confused in the transition, inasmuch as the differences of physical condition and properties are no

longer regarded as the only causes of varieties in substances; the difference in chemical properties receives more attention, the existence of elements, the producers of such properties is assumed; and thus the path is prepared which leads to the idea of chemical composition. Then we see the Aristotelian theory gradually becoming indistinct, whilst the idea of the importance of the chemical deportment and composition of bodies assumes prominence, and at last we see clearly that the differences between the substances which nature presents to us in such overpowering numbers, or which we have ourselves formed artificially, depend upon differences in their chemical composition. The idea of chemical composition, which makes its first appearance indistinctly in the history of the chemistry of the Middle Ages, now forms the foundation of the science."

The most important and interesting problem at this time, and the one upon which most attention was focused, was the chemistry of combustion. Attempts to explain the phenomena of combustion finally led to the phlogiston theory of Stahl, which dominated the science from the end of the seventeenth through the eighteenth century.

In 1675, Nicolas Léméry published his "Cours de Chimie," which soon became one of the most popular textbooks of the time and passed through thirteen editions during its author's lifetime. In it he divided all natural substances into mineral, vegetable, and animal; including in the second group plants, resins, gums, fungi, fruits, acids, juices, flowers, mosses, manna and honey; and under the third heading describing the various parts of animal bodies. This classification was quite generally adopted, and thus arose a distinct separation of mineral chemistry from the chemistry of substances occurring in plants and animals. The phlogistonists had previously opposed any such subdivision, contending that the differences observed depended upon a variation in the composition of the bodies classed under the three heads. So Becher, in 1669, argued that the same elements occur in the three natural kingdoms, but that they are combined in a simpler manner in mineral substances than in vegetable or animal. Stahl, in 1702, asserted that in vegetable as well as in animal substances the watery and combustible principles predominated, and that these ultimate constituents made their appearance when the organic compound was heated out of contact with air, water and combustible charcoal being

formed. These ideas were successfully combated by Boyle, who had shown, as early as 1661, in his "Sceptical Chymist," that the application of heat leads to quite different results depending upon whether air is present or not, and that the various residues thus obtained are unlike.

Many organic substances were discovered during this phlogiston period, but their real composition (even qualitative) remained unrecognized. For example, it was assumed that the ultimate constituents of alcohol were oil and water, or a combustible and a mercurial principle. By far the greater number of the investigations recorded were still in the inorganic field, probably for reasons already given, and also because it had not as yet been possible to prepare organic compounds synthetically. While, as has been said, many authors adopted Léméry's method of separating mineral, vegetable and animal substances, others still adhered to the old system of grouping together all acids (sulfuric with lactic, tartaric, etc.), all salts, etc.

Boyle's influence was soon effective in directing a closer scrutiny of the composition of compounds, and gradually the true elements were isolated and studied.

The discovery of the composition of carbonic acid gas by Lavoisier in 1775, and that of water by Cavendish, showed the presence of carbon and hydrogen in alcohol (1784). Lavoisier, having established the true principle upon which combustion depends, analyzed various organic substances and came to the conclusion that vegetable substances were composed generally of carbon, hydrogen and oxygen, while animal substances contained also nitrogen and occasionally phosphorus. He did not distinguish organic chemistry as a special branch of the science, or define it as "the chemistry of the compound radicals." He discussed all acids together, subdividing them into mineral, vegetable and animal.

Macquer, who was professor of medicine in the University of Paris, and a contemporary of Lavoisier, in his "Elements of the Theory and Practise of Chymistry" (English translation of 1775) discusses mineral, vegetable and animal oils together, and in the separate sections of his work devoted to vegetable and animal

chemistry divides the subject according to the method of treatment employed to obtain the substance rather than according to the character of the substance itself. Thus we have as the main headings, "Operations on unfermented vegetables," "Operations on fermented vegetable substances," and "Operations on animal substances."

Fourcroy (about 1790), however, in his well-known text-book, makes a clean-cut division, placing the vegetable acids in the section dealing with the vegetable kingdom, and the animal products all under the animal kingdom.

It should be noted that at this time carbon was supposed to exist as such in plants and animals. So Chaptal, in 1791, says:

> "Carbone exists ready formed in vegetables. It may be cleared of all the volatile and oily principles by distillation, and, by subsequent washing in pure water, it may be deprived of all the salts which are mixed and confounded with it."

In Thomson's "System of Chemistry" (third edition, 1807), marsh gas and olefiant gas are discussed with the element carbon, but the other carbon compounds are scattered under various headings where they are mixed in with inorganic substances.

In the text-books and treatises on chemistry at this period it was customary to combine mineralogy and geology with the mineral part, botany with the vegetable section, and physiology with the portion dealing with animal chemistry, while occasionally physics received as much space as chemistry in the introductory chapters.

The ætherin theory of Dumas and Boullay, propounded by them in 1815, and later adopted by Berzelius, was an adaptation of the early theories concerning the composition of organic compounds (by which they were supposed to consist of an aqueous and a combustible principle) to new conditions. In their theory, many derivatives of alcohol were regarded as compounds of C_2H_4 (to which Berzelius had given the name "ætherin"), in the same way that ammonium salts are derived from NH_3:

$$C_2H_4 \cdot HCl = NH_3 \cdot HCl,$$

$$C_2H_4 \cdot H_2O (\text{alcohol}) = NH_3 \cdot H_2O,$$

$$C_2H_4 \cdot H_2SO_4 = NH_3 \cdot H_2SO_4,$$

$$(C_2H_4)_2H_2O (\text{ether}) = (NH_3)_2H_2O (?).$$

The attempt was made to apply this system of classification to other derivatives of alcohol and even to extend it to all organic compounds; but it never won any widespread recognition.

Berzelius, in 1817, explained the difference between inorganic and organic compounds by stating that every oxidized inorganic compound contained a simple radical, while organic compounds consisted of oxides of compound radicals; and that in vegetable substances the radical usually consisted of carbon and hydrogen, while in animal substances it consisted of carbon, hydrogen and nitrogen. He therefore defined organic chemistry as "the chemistry of the compound radicals" (1843). His conception of the structure of organic compounds was a dualistic electro-chemical one, in which the organic radicals played the same rôle as the elements in inorganic compounds; thus, both electro-positive and electro-negative radicals were assumed.

Gmelin, in the first edition of his great "Handbook" (1817), says that a clear distinction should be made between inorganic and organic chemistry, but that this is a distinction which can be more readily felt than strictly defined. He describes inorganic compounds as binary compounds, the simplest consisting of compounds of two elements, a basic oxide or an acid oxide, which can again unite to form a binary compound of a higher order, *i. e.*, a salt. Organic substances, on the other hand, are at least ternary compounds, or are composed of three simple substances, generally united in less simple ratio than in inorganic. Hence, he includes in the inorganic portion of his book methane, ethylene, cyanogen, and the like. He adds, further, that organic compounds cannot, like the inorganic, be artificially built up from their elements.

Berzelius also supported the last statement, claiming that in living structures the elements obeyed totally different laws from those which regulate their behavior in the inanimate world. Organic bodies were thus regarded as the special products of a mysterious vital force and, although he admitted that bodies occurring in nature might be converted into other organic compounds by chemical reactions, he maintained that none could ever be built up from their elements. Consequently, Wöhler's production of urea from am-

monium cyanate in 1827, being an incomplete synthesis, did not immediately overthrow the vitalistic doctrine. Then too, this synthesis remained for some time the only case of the kind, and urea itself was regarded as standing halfway between inorganic and organic compounds, because of the ease with which it decomposed into carbon dioxide and ammonia.

As the result of the classic researches of Liebig and Wöhler, in 1832, upon the radical of benzoic acid, the radical theory was enlarged by both Berzelius and Liebig.

Dumas, in 1837, explained the origin of so large a number of organic compounds from so small a number of elements, by stating that the latter unite to various radicals, which occasionally act as chlorine or oxygen, and occasionally as a metal. Cyanogen, ethyl, benzoyl, etc., were therefore said to constitute the elementary bodies of organic chemistry, their elementary components only being recognized when the organic nature of the compound was entirely destroyed. It is easy to see therefore why the search for these organic radicals was vested with such interest. In fact, the discovery and isolation of these radicals became the most interesting problem of the day and led to many valuable results.

In the text-books of this date, we find practically all organic compounds grouped under the two headings of Vegetable and Animal Chemistry; very few organic substances remained in the Inorganic part. An ever increasing number of these compounds found place in the separate chapters on Carbon and its Derivatives. Thus, in the manual compiled by Webster in 1826, when lecturer in chemistry at Harvard University, we find, in addition to CO, CO_2, and other simple compounds previously discussed with carbon, also the chlorides of carbon, cyanogen, cyanogen halides, HCN, thiocyanic acid, CS_2 and thiocarbonates; in Dumas' great "Traité de Chimie" (1828) also rose oil, naphthalene, sweet oil of wine, naphtha, petroleum, turpentine, cyanic and fulminic acids. In most cases, the acids, being most important, were the first to be considered under the heading Vegetable Chemistry, then followed the other groups—oils (fatty and volatile), carbohydrates, camphors, alkaloids, etc., the rapid increase in the knowledge of organic compounds being

exhibited in the closer and more logical classification within the groups. The term "organic chemistry," to include both vegetable and animal chemistry, used by Berzelius in his "Handbook," was quite generally adopted.

In 1836, Laurent advanced his nucleus theory which, although never generally accepted, was used by Gmelin in his "Handbook," with certain alterations, as a foundation for a classification of organic compounds. According to this theory, every organic compound contains a group of atoms termed a "nucleus" or "germ." Primary nuclei consist of carbon and hydrogen, and in these the hydrogen may be replaced by other elements or groups of elements, thus giving rise to derived or secondary nuclei, analogous in composition and chemical properties to the primary nuclei. Other atoms may be attached to this nucleus, or they may quite surround it, and when these are removed the primary nucleus reappears.

In 1839, Dumas developed his substitution theory to a theory of chemical types. An advance was made in the replacement of the dualistic formulas by unitary ones.

Gerhardt's residue theory appeared at about this time. It may well be explained in comparison with the older radical theory. According to the latter, ethyl nitrate, for example, was regarded as the nitrate of ethyl oxide, $(C_2H_5)_2O \cdot N_2O_5$; while, according to Gerhardt, the combination of the nitric acid and alcohol occurs in such a manner that one compound gives up a hydrogen and the other a hydroxyl, forming water, the two "residues" then uniting to ethyl nitrate.

The discovery of the compound ammonias by Wurtz (1849) and Hofmann led to the arrangement of organic compounds on types of various simple inorganic bodies. For example, it was assumed that the hydrogen in ammonia not only could be replaced atom for atom by other elements, but also by compound radicals.

Gerhardt's type theory was really a combination of his residue theory with the older radical theory. His four fundamental types were hydrogen, hydrochloric acid, water and ammonia; H—H, H—Cl, H—O—H, $N\begin{smallmatrix}/H\\-H\\\backslash H\end{smallmatrix}$, to which Kekulé subsequently added

methane, CH_4. These proving insufficient, multiple and mixed types were invented.

So early as 1838, Gerhardt had called attention to the fact that by the action of sulfuric acid upon various substances compounds were produced in which the characteristic properties of the constituents were not present. To distinguish such, he coined the term "copulated compounds." His original views were considerably enlarged and modified by Berzelius. According to this point of view, many radicals were assumed to be composed of several simpler ones. Thus, the fact that many monobasic acids (written on the water type) could frequently be decomposed with liberation of the CO group as CO_2, together with the alcohol radical, caused the acid radicals to be looked upon as made up of CO and an alcohol radical, $CH_3 \cdot CO—O—H$, instead of $C_2H_3O—O—H$, and paved the way for the modern structural formulas.

It was Williamson who showed that the existence of compound radicals could be assumed just as well for inorganic as for organic compounds, and that organic chemistry could no longer be correctly designated as "the chemistry of the compound radicals."

With the discovery of substances common to both plants and animals, the subdivision of organic chemistry into vegetable and animal chemistry was quite generally abandoned.

Gmelin says in his "Handbook" (Vol. VII., pp. 4 and 5):

> "Carbon is the only element which is essential to organic compounds; every one of the other elements may be absent from particular compounds, but no compound which in all its relations deserves the name "organic" is destitute of carbon. . . . If we were to regard as organic those carbon compounds which have been classed hitherto among inorganic substances, namely carbonic oxide, carbonic acid, sulfide of carbon, phosgene, cast iron, etc., we might define organic compounds simply as 'the compounds of carbon'; but organic compounds are still further distinguished by containing more than one atom of carbon. . . . Hence the term 'organic compounds' includes all primary compounds containing more than one atom of carbon."

This last qualification was unfortunate, for it was soon shown that the atomic weight of carbon was 12, instead of 6, and that, therefore, methyl alcohol and formic acid contained only one atom of carbon and would be excluded from organic compounds by the above definition.

Kane, several years before (about 1840), had exposed himself to no such difficulty. In his "Elements of Chemistry" he discussed all organic compounds as carbon derivatives and prefaced this chapter with the following remarks:

"The element which is peculiarly organic and which, with the one exception of ammonia, exists in all bodies derived from an animal or vegetable source, is carbon. It is hence that I have deferred the description of carbon and its compounds until I could pass directly from it to the great variety of organic bodies of which it is the basis. With the constituents of inorganic bodies it has but an accidental connection; for, as I shall hereafter show, there is no form of carbon which has not at some time made part of an organized being."

In the great "Handwörterbuch" of Liebig, Poggendorff and Wöhler (1851), we find the following:

"Since, however, a natural boundary between organic and inorganic compounds in general does not exist, and can no longer be assumed, since we know that both are subject to the same combining laws, and since, therefore, if a separation is desired, an artificial and arbitrary boundary line must be drawn, it appears simplest to designate organic chemistry directly as 'the chemistry of the carbon compounds,' and only a few, namely the simplest carbon compounds—CO_2, CO, $COCl_2$, CS_2 and carbamic acid—are more conveniently referred to inorganic chemistry."

Kekulé later (1866) expressed himself in similar vein. He says:

"We must come to the conclusion that the chemical compounds of the vegetable and animal kingdoms contain the same elements as those of inanimate nature. We know that in both cases the same laws of combination hold good, and hence that no differences exist between organic and inorganic compounds either in their component materials, in the forces which hold these materials together, or in the number or mode of grouping of their atoms. . . . If, however, for the sake of perspicuity, a line of demarcation is to be drawn, we must remember that this boundary is an empirical rather than a natural one and may be traced at any point which seems most desirable. If we wish to express by 'organic chemistry' that which is usually considered under the name, we shall do best to include all carbon compounds. We, therefore, define organic chemistry as 'the chemistry of the carbon compounds,' and we do not set up any opposition between inorganic and organic bodies. That to which the old name of organic chemistry has been given, and which we express by the more distinctive term of the chemistry of the carbon compounds, is merely a special portion of pure chemistry, considered apart from the other portion only because the large number and the peculiar importance of the carbon compounds renders their special consideration necessary."

This change in the significance of the term "organic" chemistry

marks the passing of the old Vitalistic doctrine, and before we lose sight of it altogether, it may not be amiss to quote some interesting passages from Meldola's recent work on the "Chemical Synthesis of Vital Products." He says, among other things, that while it is quite true that we can produce in the laboratory substances identical with those formed in the living organism, in the majority of cases we cannot maintain that the syntheses are identical in their mechanism, and those who would "explain" the biochemical processes by a simple chemical equation should bear in mind the fact that "the sign connecting the two sides of the equation stands for the whole unexplored region of biochemical transmutations." We lack exact knowledge of the nature of the synthetic processes going on in the living organism, and there is little reason for believing that they have much analogy with our laboratory methods. In fact, we cannot duplicate in the laboratory the most fundamental of all these syntheses—the photosynthesis accomplished by plants, in which carbon dioxide is absorbed by an organic compound and the product decomposed with liberation of oxygen. While the author does not at all array himself on the side of the vitalists, he concludes, from the summary of experimental results recorded in his book,

"that the testimony of pure chemistry cannot, as it stands at present (*i. e.*, about 1904), be legitimately interpreted into a direct negation of Vitalism in any form. This negation may, and probably will be made possible in the future, when our chemical methods have been made to approximate more closely to the vital methods."

Until about the year 1830, it was supposed that the same element could present itself in only one form, endowed with one invariable set of properties, and that from the combination of the same elements in the same proportions, only one and the same substance could possibly result. The discovery of isomeric compounds, consequently, led to a more careful search for the cause of the difference in the properties of substances with the same percentage composition. With the establishment of the correct relations of atom, molecule and equivalent, the way was opened for the valence hypothesis, and in 1858 Kekulé said:

"I do not regard it as the chief aim of our time to detect atomic groups which, owing to certain properties, may be considered radicals, and thus to

include the compounds under certain types, which in this way have scarcely any other significance than that of type or example formulas. I am rather of the opinion that the generalization should be extended to the constitution of the radicals themselves, to the determination of the relation of the elements among themselves, and thus to deduce from the nature of the elements both the nature of the radicals and that of their compounds."

The recognition of the quadrivalence of carbon atoms and their power of uniting with each other, accounted for the existence and combining value of radicals, as well as for their constitution. The type theory therefore found a broader generalization and amplification in the extension of the valence hypothesis of Kekulé and Couper to the derivatives of carbon.

While in years gone by, as has been said, the classification of carbon compounds was mainly or exclusively according to the source from which they were obtained, in modern times the classification has been based solely upon their structural relations and entirely independent of their origin.

One of the first to adopt this method of classification was Löwig, in 1840. Gmelin, in 1848, arranged carbon compounds in his "Handbook" according to the number of carbon atoms they contained, and subdivided them on lines similar to those suggested in Laurent's nucleus theory, as already mentioned.

Schiel, in 1842, remarked upon the fact that alcohol radicals form a simple and regularly graded series of bodies, of which the properties as well as the composition exhibit corresponding regular gradations, and he predicted the existence of other similar series. Shortly afterward, Dumas pointed out that the fatty acids constitute such a series. Gerhardt, in his "Précis de Chimie Organique" (1844), collected a large number of such groups, gave to them the name "homologous series," and distributed them under the general divisions suggested by his type theory. This recognition of homologous series as the units in classifying organic compounds was a great step in advance, simplified the classification enormously, and was very fruitful in stimulating investigation to discover other similar series.

The terms "fatty" and "aromatic" chemistry appeared about 1858. At first used in more restricted sense, they were gradually

extended until the former covered all acyclic compounds and the latter nearly all cyclic. This subdivision of organic chemistry has been generally adopted (with few exceptions) ever since. More recently, it has been found advisable, particularly in the larger text-books, to split up aromatic chemistry into carbocyclic and heterocyclic. So that we now have the three classes, fatty (or aliphatic), carbocyclic (or isocarbocyclic), and heterocyclic. And yet this classification is no longer satisfactory, for there is no sharp dividing line between straight-chain and cyclic compounds, the one merging gradually into the other. Certain cyclic structures (as the ethylene oxides, lactones, lactames, imides, etc.) are invariably discussed under fatty chemistry, and certain straight-chain compounds (like the olefin terpenes and their derivatives) are generally taken up under aromatic chemistry, while the alicyclic compounds, as their name indicates, form the natural transition from aliphatic to cyclic structures.

With the filling in of the gaps heretofore existing between aliphatic and aromatic chemistry, the time seems appropriate for a change in our classification of carbon compounds which shall recognize the essential unity of the subject, and no longer give the impression that organic chemistry is composed of three varieties of chemistry—fatty, carbocyclic and heterocyclic.

The method which appeals particularly to the writer, and which he has followed with his classes at Columbia University for the past ten years, is to begin with the hydrocarbons, as the simplest carbon compounds, and discuss in succession the various series of hydrocarbons, saturated and unsaturated, acyclic and cyclic, before passing on to the next group. After a careful consideration of these fundamentally important compounds, other classes of carbon compounds are taken up in similar manner; all of the simple halogen derivatives being considered together, all the nitro bodies, all the alcohols, and so on. All other classes are very conveniently regarded as derivatives of the hydrocarbons. With a knowledge of the properties of the various series of hydrocarbons, the study of their derivatives then resolves itself chiefly into the following questions: (1) What are the characteristic properties of the group under con-

sideration (be it halogen, amino, carboxyl, or any other group)? (2) In what manner are its properties influenced by the hydrocarbon nucleus to which it is attached, and by the other groups present? (3) How are the properties of the entire molecule likely to be affected by the introduction of such an element or group? To take a single case, by way of illustration, the simple hydroxyl derivatives of the hydrocarbons are numerous and important, and certain well defined characteristics cling to the hydroxyl group irrespective of the particular hydrocarbon nucleus to which it is attached. Thus, its hydrogen may be replaced by metals (giving alcoholates or phenolates), by hydrocarbon radicles (giving ethers), by acid radicals (giving esters), or the entire hydroxyl may be replaced by a halogen by acting upon it with a phosphorus halide. That the behavior of this hydroxyl group is influenced, however, by the hydrocarbon nucleus to which it is attached, can be seen at once by comparing a phenol with an alcohol. Further, the presence of the hydroxyl group alters the properties of the entire molecule, as appears immediately when we compare the behavior of benzene and of phenol towards bromine, nitric acid, oxidizing agents, and so forth.

In this way, the characteristic properties of the different substituents may be firmly fixed in the mind, as well as the general nature of the various classes of organic compounds, and the student learns to associate certain chemical reactions with certain chemical structures, and to reason intelligently from a given structural formula as to the chemical behavior of the substance, whether he ever heard of the compound before or not, thus learning not only to deduce correct constitutional formulas, but also to grasp at a glance the chemical properties summarized by such formulas.

This method of classification saves an immense amount of repetition and brings home very clearly the fundamental properties and relationships of organic compounds, as well as the application of these properties in analytical and industrial chemistry. Another advantage which follows from this arrangement, is the manner in which it lends itself to laboratory illustration. As all compounds containing the same substituting element or group are

discussed together, examples for laboratory practice may be drawn from either the acyclic or the cyclic field.

The author claims no originality for this suggested classification, except so far as certain details are concerned, for it was recommended and adopted so long ago as 1864 by that distinguished Russian chemist, Butlerow, in his "Lehrbuch der organischen Chemie," and has won adherents in this country in Professors W. A. Noyes, Kremers, and possibly others. My reasons for presenting it at the present time are the evident need for some change in our present system, brought into the foreground by the approaching publication of the new edition of Beilstein's monumental "Handbuch der organischen Chemie" and the creation of national commissions on the nomenclature of organic compounds, and my firm belief, as the result of experience, that the adoption of such a system will aid in inspiring and stimulating greater interest in the study of organic chemistry.

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